VOL. IX. NO. 1. JANUARY, 1902

THE PSYCHOLOGICAL REVIEW.

THE WORLD AS MECHANISM.

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The analyses of the psychologist and of the metaphysician reveal to us that the real world in space and time is an orderly system of things given in terms of touch and movement sensations. This is the world of matter in motion which the science of mechanics attempts to describe to us. It is quite possible to treat of it intelligently without being either psychologist or metaphysician, for one may confine oneself to certain aspects of it without attempting to discuss certain others. When a physicist loosely describes matter as 'everything that one can touch,' and then busies himself with the changes that take place in the world of matter, ignoring all epistemological problems, he confines himself to a definite field of investigation, and the results he obtains within that field need not be at all vitiated by the fact that he neither raises nor suggests certain other questions with which other men busy themselves. Without leaving the plane of the common understanding, he may ask himself whether he is to look upon the material world as through and through a mechanism, or whether he must abandon this conception as being unsatisfactory. He has a right to expect that the arguments *pro* and *con* will be such as to appeal to men of intelligence who are not devoted adherents of this or that metaphysical theory.

Notwithstanding the fact that a series of eminent names may be cited as favoring the opposite doctrine, the statement does not appear unwarranted that the man of science, as such, is

coming to incline more and more to the view that the changes which take place in the world of matter form an unbroken series and are all explicable according to mechanical laws. It ought to be frankly admitted by everyone that the material world is not *known* to be such a system. We may, indeed, conceive it to have swept through an unbroken series of changes, from the cosmic mist in which our ignorance looks for its beginnings, to the organized whole in which vegetable and animal bodies play their part; but, even as we call before us the vision, we realize that it is revealed only to the eye of faith, and is but dimly discerned through the obscurity which enshrouds it. Even if we leave out of view the difficulties connected with the structure of the atom and the nature of the ether, we are forced to admit that scarce so much as a beginning has been made in the direction of a mechanical explanation of the combination of atoms into molecules and the origin of the kinds of matter of which, as the chemist informs us, our world is made up. Given, too, the chemical elements and the laws of their combination as empirically known to the chemist, we still search in vain for an explanation of the phenomena of living organisms, and fail to account for their appearance upon this planet. Chemistry, physics, biology—these are as yet relatively independent realms, and it remains for a perhaps far distant future to give them all a solid basis in mechanics, and thus to unite our present fragmentary glimpses into the nature of things into a reasonable and comprehensive whole. We have a collection of sciences whose relations to each other are not clearly seen. We have not yet a science which can string on a single thread the beads that we have with such labor collected together.

But it is one thing to admit our present ignorance, and it is quite another to maintain that it is, in the nature of things, ultimate and irremovable. The steady growth of science encourages those who are imbued with the scientific spirit to hope that, in our knowledge of nature, discontinuity will gradually give place to continuity, and that there will become more and more clear before our eyes an orderly mechanical system, the successive stages in the evolution of which will not have to be accepted as inexplicable fact, but will be seen to be the appro-

priate steps in a series of changes, the inevitable succession of which we may infer with confidence, and which we are unable to comprehend only where we are still hampered by our ignorance. That this faith in the mechanism of nature is justified cannot be proved by the philosopher in his closet. It can be proved only by the actual extension of our knowledge of nature, and until this has taken place, the doctrine can be no more than a working hypothesis. It is, however, sometimes urged that it should not be held even as a working hypothesis, and various considerations are brought forward to prove that the doctrine is inherently absurd. Upon certain of these I shall dwell briefly in what follows.

I.

If one may judge by the comments and reviews in a considerable number of the more popular journals, many persons have followed with pleasure the recent attempt made by Dr. James Ward to prove that the science of mechanics is no true science at all, and that its fundamental concepts, when carefully examined, turn out to be self-contradictory and absurd.[1] I do not propose here to criticise Dr. Ward's reasonings in detail. I wish merely to point out that they seem to arise, in part, at least, out of a misconception of the nature of the science of mechanics and of the foundations upon which it rests. That even in dealing with the most common things about us, the movement of a rolling ball, the raising of a weight with the aid of a lever, the rotation of a wheel upon its axle, all our measurements are only approximately correct, and fall short of the exactitude demanded by scientific theory, no one need trouble himself to deny. That certain scientific concepts, which are undoubtedly useful within a given sphere, may, when carried over to or beyond the confines of our knowledge, and employed in those somewhat fanciful constructions by the aid of which the man of science hopes to win at least a glimpse of truths which he must frankly admit lie beyond his horizon—that such concepts may sometimes be found inadequate to such uses, need not be found surprising. We all listen with more curiosity than

[1] 'Naturalism and Agnosticism,' Part I.

conviction to what certain speculative minds have to tell us of the ultimate constitution of matter; and those who speak, if they be men of sense, do not pretend to be clothed with the spirit of prophecy. But all this has nothing to do with the question whether a science is a true science, and rests upon a reasonably secure foundation.

It should never be forgotten that the science of mechanics, like other sciences, has its foundation in our common experience; that it is merely the systematization, refinement and extension of our ordinary knowledge of things and their motions. The savage, who uses a stick to pry a stone out of its setting, the boy who throws a bit of coal at a cat, even these have made a beginning in the knowledge of a mechanical system of things. That no little advance has been made from such a beginning is patent to anyone familiar with contemporary science. The notion of mechanism is a perfectly familiar one, and to it we constantly turn for an explanation of changes which we perceive to be taking place in the world about us. Whatever may become of the doctrine of atoms and molecules, it remains true that we can calculate with some degree of accuracy the position of the moon with reference to the earth on a particular day and hour, and we can trace with some accuracy the path of a projectile. Whether we may not justly expect to find in the notion of mechanism the explanation of all the changes that take place in the material world, is a question that it is by no means absurd to raise, even when one is not at all in a position to prove that all changes in matter are mechanical. One may raise the question, and may be inclined to give it an affirmative answer, although one be in doubt whether any proposed theory of the intimate constitution of matter be the correct one. Very early in the history of speculative thought it occurred to men's minds that those things which, by reason of their minuteness, are concealed from our view, might be reasoned about by analogy with those things which are more open to inspection. With the principle itself we can have no quarrel. We act upon such principles in every department of human thought. It is, of course, important that we should not reason loosely, and should not too hastily arrive at conclusions. And if any as-

sumptions which we have been impelled to make should turn out upon closer inspection to entail consequences which we cannot accept, we should know how to repudiate those assumptions without tossing overboard with them that whole body of observed facts and well-grounded generalizations which have established their right to be regarded as a science, if only an imperfect one. There is such a body of facts and generalizations that constitutes the science of mechanics. To laugh at this science because it has its limitations is absurd, and it is a complete misconception to suppose that a science must be completed before it can have a foundation. In the present instance, it is the apex of the pyramid that is hid in clouds, not its foundation, for this lies in plain view, and no man can afford to despise it.

To the metaphor of the pyramid it may be objected that, when one is dealing with a science, a view of the apex may very materially modify one's notions of the extent and outline of the base. This is quite true. A science does not grow by mere accretion. The discovery of a new truth may necessitate a modification of generalizations which seemed amply justified when our information was more limited. The Newtonian laws of motion, for example, have justly been regarded as a triumph of modern science, and as lying at the very foundation of the science of mechanics. Nevertheless, we are now told[1] that they will probably have to be modified if we wish to describe accurately the material universe as it seems to be revealing itself to the inquisitive eye of the modern investigator. Does this mean that the observations and reflections which resulted in those laws are to be regarded as valueless, and the laws themselves to be repudiated as utterly erroneous? By no means.

It means only that a truth imperfectly apprehended has come to be more perfectly apprehended. Such a growth in human knowledge is not revolution; it is entirely in harmony with the natural and normal development of the sciences. What are commonly called their fundamental principles or concepts are not fundamental in the sense that they must be definitely

[1] Pearson, 'The Grammar of Science,' 2d ed., Chap. VIII.

established and placed beyond the possibility of being called in question, before the science can be built up at all. Such principles or concepts are the ideal of a completed science, if such a term may be used. They are not to be found in a science in the making. They are the goal toward which a science strives, and not the ground which it actually feels beneath its feet. Hence one may freely admit that men of science are not at one touching the final definition of matter, and are not agreed upon the proper formulation of the laws of motion, without on that account being compelled to deny that there is such a science as mechanics, and that in it we find a satisfactory explanation of a vast number of the changes which we observe to be taking place in the world. And one may make these admissions without being compelled to abandon the hope that, with the extension of human knowledge, a vast number of other changes, which cannot now be seen to find their explanation as these do, may be found to fall in the same general class, and may become luminous with a significance now denied to them. It is sheer dogmatism to insist that the material world cannot be a perfect mechanism, merely on the ground that, in the present state of our knowledge, it cannot be proved to be such. What we should ask ourselves is this: What, on the whole, is it reasonable for us to believe, and with what degree of assurance should we believe it? He who is accustomed to weigh evidence, and who realizes the limitations of our actual knowledge, will take his position on such a subject tentatively, and will hold himself in readiness to abandon it when good reason is adduced for his doing so.

The detailed discussion of Dr. Ward's strictures upon the science of mechanics may safely be left to the intelligent student of natural science. A number of them are, I think, clearly open to objection upon purely scientific grounds. But there is one general consideration touching the attitude of Dr. Ward and of a sufficiently large number of other persons toward the mechanical view of the system of nature, that is of no little significance, and that has not, I think, received due emphasis. It is this: The energetic rejection of the doctrine that the material world may be regarded as a perfect mechanism appears

to arise (if one may judge by what is written upon the subject) out of the conviction that such a view of the world militates against certain beliefs to which men cling with a good deal of energy and which they relinquish with reluctance. We do not find that attacks upon the conception of mechanism are wholly destructive in their aim. Those who cannot find in mechanics an explanation of the changes which take place in the material world, are inclined to find such an explanation in the action and interaction of minds. They do not merely abandon a proposed view of nature because they find it unsatisfactory, and content themselves with holding no view at all. They abandon one view to take up with another. It seems just to ask oneself whether, if there were the same emotional bias against the second view that appears to exist against the first, it would be found so satisfactory as many seem to find it? Are there no difficulties connected with the second view? Do we there find everything clear and comprehensible?

Let us suppose, for example, that we discover, upon reflection, that the conception of matter remains to us obscure; that we can gain no very clear notion of what is meant by mass: that we are more or less in the dark as to how the idea of causality can be connected with the changes in the material world; that the laws of motion, as at present formulated, do not seem to us to account satisfactorily for the behavior of all material particles in the presence of each other. Shall we on this account repudiate the science of mechanics, and give up all attempts at a mechanical explanation of the changes which take place in the world of matter? If so, what should we do in the case of mind? Are there no disputes as to the ultimate nature of the mind? Is there a science, or even the beginning of a science, that sets forth with any approach to clearness the relation of mind to matter, and the method by which minds act upon material particles or upon each other? Is it more evident what is meant by causal efficiency when one speaks of minds than when one speaks of masses of matter? 'Intersubjective intercourse' is a sounding phrase that calls our attention to the fact, recognized in our common experience, that, *in some sense*, minds stand in relation to each other. But in what sense? How

can a mind be related to another? Has the vague knowledge of the plain man really been replaced by something that has a right to be regarded as science? Surely the science of mechanics, unsatisfactory as it may be, has progressed far beyond our knowledge of mind, of its relations to matter, and of its relations to other minds. Here we see in a glass, darkly; each man is busied with his own speculations, and they are worth all the labor which he devotes to them. But a science we have not, unless we extend the meaning of the term so as to cover those tentative gropings for the truth which precede established knowledge. To find fault with the science of mechanics, and to take up with the vague notions which men have of minds, the activity of minds, the relation of minds to matter, and their relation to each other, is about as sensible as it would be to reject the refinements of the developed science of mechanics and take up with the crude mechanical notions possessed by the uneducated. That material things act upon one another, and that minds act and react, the plain man does not doubt. He sees nothing incomprehensible in the premises on the one side or on the other. It is the philosopher who becomes conscious of the inadequacy of his conceptions, and whose reflections sometimes tempt him to reject them altogether. But to treat one class of conceptions in the critical spirit of the philosopher, and to accept the other with the naïveté of the unreflective, is surely inadmissible. If, then, it is right to lay great emphasis on the difficulties which suggest themselves when one undertakes a critical investigation of the fundamental concepts of the science of mechanics, it must be equally just to emphasize the difficulties which arise when one endeavors to make quite clear to oneself what is meant by minds, their relation to material things, and their relation to each other. If one insists upon clearness and consistency in the former field, and is content to get along without it in the latter, it must be either that, in the latter field, the attainment of exact knowledge is looked upon as, in the nature of things, hopeless; or that the deficiencies of our knowledge are hidden from us by an emotional bias that inclines us strongly to adopt certain doctrinal statements whether they are clear to us or not.

II.

Since, therefore, it is possible for a science to be a true science before it has attained ideal completeness, it does not seem wholly unreasonable for one to assume, as a working hypothesis, at least, that the material world is a mechanism all the changes in which can be accounted for without passing beyond it to something else. Let us make such an assumption. In the orderly succession of the states which constitute the life-history of this organization we have the physical order of causes and their effects. It is, of course, clear that our knowledge of physical causes and their effects must be imperfect as our knowledge of the world-mechanism is imperfect. The boy who strikes a dog with a stick recognizes the answering yelp as a consequent, to which the movement of the stick is a corresponding antecedent. The physiologist interpolates an extremely complicated series of occurrences between the two, and regards the blow as by no means a proximate cause, while admitting it as a member in the causal nexus. Both recognize the relation of cause and effect, but to the latter the whole system has become a vastly more complicated thing than it is to the former. And the metaphysician, who may come to the conclusion that there is no assignable limit to a possible increase in the minuteness of our knowledge of the real world and its changes, may not unreasonably deem it absurd to use the expression 'proximate cause' in any but a relative sense. Still, he has the right to use it to indicate an antecedent which, in the actual state of our knowledge, seems to be nearest to a given consequent.

In the relation of cause and effect, when thus conceived, there seems to be nothing very occult or mysterious, though one must, of course, admit that we have but an imperfect knowledge of causes and their effects. The conception of causality seems, however, to be a stone of stumbling to some, and it is well worth while to devote some time to its analysis, notwithstanding the fact that it has been discussed, and well discussed, by various writers. In connection with it there appear to arise some very general misconceptions, and misconceptions which may materially modify one's view of the mechanism of nature.

Our text-books on logic warn us that the custom, which prevails in common life, of picking out that element in the total antecedent of a given occurrence which happens to be for some reason of peculiar interest or importance, and of calling it *the cause*, may easily occasion mistaken notions of cause and effect. We point out that Smith is the cause of the accident that happened to Jones, in that he handled his gun carelessly. Jones himself we do not speak of as contributing to the result. Yet it is quite clear that a man cannot be shot *in absentia*, and the bodily presence of the injured man was an indispensable part of the antecedent if the occurrence were to take place at all. The total antecedent of a given occurrence may be a thing of infinite complexity, and yet we are usually content to adduce one or two elements in it and overlook the rest. We attribute an outbreak of the plague to the visit of a ship from an oriental port, the prosperity of a country to the dominance of a political party, a happy marriage to an accidental meeting in a country house, a deadly feud to a chance word spoken in an unguarded moment. Even so we speak of a general as gaining a battle, and of a wise ruler as giving peace and prosperity to his subjects. There is no objection to our thus speaking in common life, but it is necessary to observe that, when we pass to the scientific contemplation of the order of nature and inquire regarding the explanation of given collocations of matter and motions of matter, we must view things with a more impartial eye and must not overlook the most humble contributor to the total result.

Again. In common life we are accustomed to emphasize the distinction between agent and sufferer. At times we regard ourselves as actively bringing about changes in other things, and at times we deplore the fact that external things bring about changes in us. We look upon ourselves as active when we move along the street in pursuance of a desired end, and as wholly passive with respect to the falling tile that unexpectedly interrupts our progress. This distinction we carry over to things inanimate, and the notions of activity and passivity are apt to become more or less confused with those of cause and effect.

But in the conception of nature as mechanism this distinction between active and passive wholly vanishes. The moving billiard-ball comes in contact with the ball at rest. The former comes to rest and the latter is set in motion. We are at first inclined to regard the one as active and the other as the passive recipient of its activity. But a little reflection and the most elementary knowledge of mechanical laws make clear to us that the second ball has affected the first quite as much as the first has affected the second. A series of changes has taken place in the spatial relations of certain masses of matter, and it is only through misconception that we can regard a single mass of matter as responsible for the series of changes as a whole. When we do so we are carrying over to a field in which it has lost its significance, a conception which has its legitimate application only in another field.

The same reasoning may be applied to the case of the boy striking the dog. If we will regard boy, dog and stick as merely a part of the material system of things, as collocations of matter the changes in which take place according to mechanical laws, it is impossible to look upon the boy as active and the dog as the passive recipient of his action. When we do so regard them we are employing conceptions which have a significance only in the subjective world of desires and volitions, a world with which we have nothing to do so long as we confine our attention to the material universe and its motions. To the eye with its field of view thus circumscribed, nothing is present save certain groupings of material particles which pass through a series of changes in their relative positions. The notions of activity and passivity have disappeared, but not so the notions of cause and effect. The changes through which the whole system passes are explicable according to the laws of mechanics, and each antecedent condition is the cause of the one which immediately follows it. The relation of cause and effect is a temporal one, and marks the order of the successive states in the life-history of the system; it is not a spatial one, which separates off one part of the system from another part. In other words, the boy and the stick cannot be made in some sort an antecedent, and the dog a consequent; but boy, stick and dog

are all antecedent, and are all consequent as well—the former at the one instant, and the latter at the next. The erroneous popular judgment which would make the boy the sole cause of the dog's yelp, seems to arise from a double error: the attention is fixed upon a part of the total antecedent to the exclusion of the rest; and there is present the mistaken notion that only that can be a cause which is 'active.' The popular judgment is not without its justification from a practical point of view. It is not a mere accident that men come to think and speak thus. Nevertheless, the popular judgment is shot through with misapprehension and confusion, which should, in scientific discussions, be eliminated. The notions *cause* and *activity*, *effect* and *passivity*, should be carefully divorced from one another when we concern ourselves with an exact description of the changes which take place in the material world. That the notions activity and passivity are of the utmost significance in their proper field, one may freely admit. But it is important to bear in mind that, when we are studying the successive positions of matter in motion, we have nothing to do with them at all.

It is an imperfect apprehension of the distinction between causality and activity that has misled certain writers, scientific and anti-scientific,[1] into thinking that natural science should drop altogether the notion of causality, and in place of an *explanation by a reference to causes*, substitute a *description* of the orderly series of changes that take place in the world of matter. Just so long as he confuses causality with activity, will the student of mechanics, who sees clearly that the notion of activity has no place in his science, be inclined to deny that he has to do with causes and their effects. It is because he still thinks of "an explanation by a reference to causes" as something occult and mysterious—as a procedure akin to the blind gropings behind the veil of phenomena popularly attributed to the metaphysician—that he repudiates such explanations altogether and confines himself to what he calls 'description.' But it is unwise to discard terms which for centuries have served a useful purpose, which are firmly rooted in men's minds and are fairly well

[1] *E. g.*, Mach, 'Popular Science Lectures,' English trans., pp. 253–254, and Ward, 'Naturalism and Agnosticism,' Lect. II. and XVII.

understood even by those who cannot subject them to careful criticism, and which have no satisfactory equivalents but leave a gap when they are discarded. For such terms one cannot substitute terms with other associations without giving rise to suspicion and misunderstanding. It is far better to correct popular misconceptions of the proper significance of words in common use, and point out how such words may find their appropriate application. To insist that science has nothing to do with the indication of causes and their effects, when for centuries that has been supposed by its votaries to be its chief occupation, can only occasion bewilderment. To show, on the other hand, that there has been some error as well as some truth in the popular apprehension of the natural order of causes and their effects need not have this unfortunate result.

That the relation of cause and effect has in the history of science been but imperfectly grasped is sufficiently clear. To see how dimly it has been apprehended even by men of commanding genius and thoroughly imbued with the scientific spirit, it is only necessary to read what Descartes has to say of effects as contained 'formally' or 'eminently' in their causes. Where, he asks, can an effect get its 'reality' if not from its cause? and he appears to think of this 'reality' as a measurable stuff that can be passed over from cause to effect.[1] Notwithstanding all this, Descartes had no mean conception of the material universe as a mechanical system, and no one can accuse him of more than a partial misconception of what is meant by the natural order of causes and effects. Indeed, one is tempted to say that he had a better notion of this order than David Hume, notwithstanding the fact that the acute analysis of the latter stripped away the mystical envelope that had before shrouded the conception of the causal relation, and laid bare the fact that causality means antecedence and succession, and is not a something in its nature obscure and incomprehensible. It seems rather natural for men, when an accession of light reveals some defect in a conception in which their mind has heretofore rested content, to go further in their reconstruction than they are justified in going. In their desire to reject error they not in-

[1] 'Méditation Troisième.'

frequently throw away with it more or less truth. To Hume the cause and effect relation becomes one of mere antecedence and consequence, with an undue emphasis upon the 'mere'; and he finds it in no sense 'necessary' that effects should follow from their causes. In following his argument we are everywhere struck with the fact that the world as an orderly system, a mechanism subject to invariable law, disappears from our view, and our attention is occupied instead with certain subjective conceptions which concern rather the psychologist than the student of physical science—such conceptions as custom, habit, expectation of the constancy of nature, which last Hume discovers to be a tendency for which no reason can be given.[1]

The reader of Hume feels the solid world crumble away beneath his feet. He has been accustomed to think that only his own ignorance stands in the way of his discovering why a particular effect follows from its cause. He now learns that the fact must be accepted blindly. Certain phenomena in our experience follow certain others, or, at least, have followed them, and we expect this to happen again. This expectation is something for which no reason can be given; and yet he cannot help feeling that it is highly desirable that some reason should be forthcoming. The relation of cause and effect appears to be arbitrary and incomprehensible. He has, furthermore, been accustomed to believe in natural necessity as something very real, and it has been to him a guarantee of the stability of the order of nature. He is now told that the connection of causes and their effects is in no sense necessary. Only a Hume can live contentedly in a world which thus resolves itself into mist and leaves one suspended in the void.

The error which lies at the root of such misconceptions is identical with that upon which I have commented a little above. It is the error of incomplete analysis. Just as the disinclination to use the word 'cause' in giving an account of the order of

[1] 'Inquiry Concerning the Human Understanding,' §§ 4–8. This is, I think, a fair characterization of the spirit of the Humian doctrine. It would, of course, be unjust to maintain that Hume recognized no distinction between antecedents in general and antecedents that may be regarded as the causes of particular occurrences. See his 'Rules by which to judge of Causes and Effects,' 'Treatise of Human Nature,' Part III.

nature has its origin in an imperfect apprehension of the fact that the notions of cause and activity are quite distinct from each other, and that there is no reason why, under certain circumstances, the one should not be taken and the other left, so the notion that the connection of causes and effects is a something inexplicable, which must simply be accepted without hope of explanation, arises from an imperfect apprehension of what science has done and is doing in the discovery of the causal relations of things, and also in a misconception of what the word *explanation* properly means. In the mechanical system of things of which I have been speaking in this paper it is probable that no change whatever can take place the total antecedent of which can be sought in only a part of the system. In other words, the whole condition of things at any given instant is due to the whole condition of things at the instant preceding. Nevertheless, science is not shut up to a knowledge of causes so broad and so vague as this. It is quite possible to pick out with some degree of accuracy individual antecedents and relate them to individual consequents, recognizing the fact that, although other antecedents may not have been without their influence in the result, nevertheless this influence is a negligible quantity and may without inconvenience be overlooked. Were it not thus possible to isolate certain factors in the total antecedent and connect them with certain factors in the total consequent, the science of mechanics could never have arisen. The method by which it arrives at its results has been clearly described,[1] and it is unnecessary for me to enter into the subject here. It is, however, important to note that this singling out of particular antecedents and connecting them with particular consequents gives rise, in the mind even of the student of mechanics, to a distinction between antecedents in general and that particular antecedent which, in a given instance, he connects with a particular consequent. Perhaps I should say keeps alive the distinction, for it exists in common thought as well as in science; the difference being that, whereas the student of science recognizes the fact that many antecedents not obtrusively present may contribute to a given result, and realizes the

[1] *E.g.*, Pearson, 'The Grammar of Science,' Chap. VIII.

difficulty of disentangling the threads in the skein, the plain man usually has his attention so taken up with the one thing that he settles upon as the cause of a given phenomenon that he quite overlooks the many concurrent causes which are less prominent or less interesting. In either case there is a recognition of the distinction between a 'mere' antecedent and that particular antecedent which is recognized as a cause. The obliteration of this distinction cannot but result in confusion. It is, consequently, with a good deal of satisfaction that the reader of Hume turns over the pages of the text-books on logic to discover that not every antecedent is a cause, but only a 'necessary' or 'indispensable' antecedent. It seems, then, that it is misleading to speak of the relation of cause and effect as that of *mere* antecedence and succession. There appear to be antecedents and antecedents; those which are, in a given instance, to be taken as causes, and those which are not.

III.

But what is a 'necessary' or 'indispensable' antecedent? Has not Hume pointed out that the relation between a cause and its effect is in no sense necessary, and that it is quite conceivable that the natural order of things should not be what it is? This latter question one may answer by showing that Hume wrote as he did because, while be recognized a distinction between two conceptions, he did not recognize it with perfect clearness. He saw that an effect is not contained in its cause as the conclusion of a syllogism is contained in its premises. Natural necessity, he saw, was not logical or mathematical necessity, and seeing this he felt impelled to deny that there is such a thing as natural necessity at all. In this he was wrong. The word necessity has, and has had for centuries past, two distinct meanings, and no man has a right to throw away one of them merely because it is not the other. To show that a given antecedent is a 'necessary' antecedent or cause, it is not necessary to show that the consequent is logically contained in it and cannot be denied without self-contradiction. It is only necessary to turn to the inductive logic and see whether there is

good reason to believe that the more or less complete elimination of other antecedents will leave this relation of antecedent and consequent virtually intact. The necessity of nature is but another name for the orderliness to be discovered in the system of things, and it is a total repudiation both of the knowledge of things which obtains in common life and of the more exact knowledge characteristic of science, to maintain that we cannot attain to a more or less detailed acquaintance with this world-order. It is not the duty of the metaphysician to show what antecedents are 'necessary' or 'indispensable.' It is the duty of the investigator of nature; and he can fulfil this duty perfectly well without paying the least attention to those mystical notions of causality which have in the past introduced a needless obscurity into human thought.

The relation between cause and effect is, therefore, a necessary one, in an intelligible sense of the word, and the denial of this necessity can only result in shaking that wholesome confidence in the order of nature possessed in some degree by the unlearned and in a higher degree by those whose knowledge of nature is more exact and extended. Sometimes this denial proceeds from a desire to remove that feeling of apprehension and even repulsion which arises in many minds at the thought of this gigantic mechanism which seems to sweep through its series of successive conditions with the impassivity of fate—a world in which even a sparrow cannot fall to the ground except according to law, but one in which the dance of an atom, the fall of a sparrow, the death-struggle of a man, appear to have one and the same significance, and to be summed up in those more or less complicated formulæ which describe the motions of material particles with reference to each other. Even so keen a man as Professor Huxley tries this method of soothing the anxieties of those who contemplate such a world with discontent, and suggests that if we will try to eliminate from our thoughts of the order of nature the notion of necessity, and will bear in mind that we are dealing with the mere relation of antecedence and consequence, we shall feel rather better.[1]

It is quite true that to the unreflective there may seem to be

[1] 'Methods and Results,' N. Y., 1893: 'On the Physical Basis of Life.'

something less august and inevitable in the succession of changes which take place in the material world when one has denied necessity to nature and has elected to regard what takes place before one's eyes as a mere play of antecedents and consequents. The starch appears to be taken out of the fabric; it hangs more limp and diaphanous. And yet, what has one gained? The pattern is precisely what it was. If it was ugly then, it is ugly now. Figure succeeds figure in the same inevitable order, and he who had reason for complaint before, has lost none by the change. The word *necessity* he has found unpleasant, and someone has obligingly given the thing a new name. Even so may the trembling householder decide to call the midnight marauder a visitor, and feel reassured and comforted. Meanwhile the man has suffered a real loss. He has lost sight of a useful distinction, and the order of nature has come to seem to him less stable and dependable than it was before.

It is, then, through an incomplete apprehension of what is properly meant by natural necessity that one is lead to deny necessity to the relation of cause and effect. And it is through a misapprehension of what is meant by *explanation*, that one is led to maintain that it is impossible to explain why certain causes should be followed by certain effects.[1] It was remarked

[1] I know no better illustration of the Humian exenteration of the notion of causality than that presented in the fourth chapter of Professor Pearson's *Grammar of Science*. He discards the idea that a cause is the occult and mysterious thing that has sometimes passed by that name. He agrees with Mill in thinking that causation is 'uniform antecedence.' But he finds it necessary to insist that the relation of cause and effect is not a *necessary* one (2d ed., pp. 113, 116, 118, 119), and he reiterates the statement that science, in discovering causes and effects, does not *explain* things: "Mechanical science no more explains or accounts for the motions of a molecule or of a planet than biological science accounts for the growth of a cell" (p. 115); "in no single case have we discovered *why* it is that these motions are taking place; science describes how they take place, but the *why* remains a mystery" (p. 120); "when we say that we have reached a 'mechanical explanation' of any group of phenomena, we only mean that we have described in the concise language of mechanics a certain routine of perceptions. We are neither able to explain why sense impressions have a definite sequence, nor to assert that there is really an element of necessity in the phenomena" (p. 116). It seems odd that Professor Pearson did not see that, if science (in the broad sense of the word) had really succeeded in finishing her task, there ought to be no *why* and no *mystery*. They disappear by absorption into the *how*.

by Immanuel Kant that it requires some sagacity for a man to know what questions he may safely ask. The remark was a wise one. There is a sense in which it is proper to ask for the explanation of this or that occurrence, and there is a sense in which it is not. Both in common life and in science we are constantly seeking an explanation of what comes to pass, and are constantly finding certain explanations satisfactory. The fall of the apple to the earth, the motion of the moon in its orbit, the ebb and flow of the tides, all these we regard as explained when they are seen to be illustrations of the laws of mechanics. The particular occurrence in question is found to have its appropriate place in the mechanical world-order, and we should rest content with this, for this is explanation. But if we will go on to insist that the whole mechanical system is a something to be accepted as inexplicable fact, we deserve any unhappiness that such reflections may occasion us. We extend the meaning of the word explanation quite beyond what is legitimate either in common thought or in science, and then complain that we lack an explanation of something, sadly electing to regard this something as 'brute fact.' This is not a recognition of the truth that no explanation can sensibly be asked for; it is a foolish insistence upon the fact that none is forthcoming, and, of course, carries with it the suggestion that it would be highly desirable if one *were* forthcoming. 'Brute fact' means fact that stands in need of explanation and appears to lack it. To call the system of things as a whole 'brute fact' is simply misleading.

IV.

Such reflections as the above should, I think, serve to set aside certain of the objections which some may be inclined to urge against the world as mechanism. If the conception of mechanism seems to us absurd, it is because we imperfectly comprehend what that conception is, as it is gradually growing clearer to science. If we deny the existence of material causes, it is because we confound the notions of causality and activity, or erroneously assume that a cause can only be something occult and mysterious, which must be eschewed by science.

If we repudiate natural necessity, it is because we fail to perceive that the word 'necessity' is an ambiguous one. If we insist that science cannot offer an explanation of the occurrences in the material world, it is because we give the word 'explanation' an unjustifiable meaning.

It is, however, quite possible for one to avoid these errors and yet to feel very dubious about yielding assent to the doctrine that the world of matter is a perfect and independent mechanism, every change in every part of which must find its whole explanation in the system itself. There are those whose mental vision can trace the paths along which the primitive elements of the universe have travelled out of their inconceivable abysses, when they came together to form those aggregates of matter which ultimately became worlds, and can find them written in the book of what was to be according to invariable mechanical law. They can see, with the eye of faith, the results of mechanical causes in the birth of the chemical elements, and the emergence of the new kinds of matter which result from their combinations. If they cannot prove that they are here in the presence of a veiled mechanism, they at least do not find it hard to believe it, and they can indulge a more or less lively hope that it may some day be granted them to have a glimpse behind the veil. But when they pass from the inorganic to the organic world, and stand in the presence of life, they find it difficult to believe that an increase of knowledge will show that here, too, the conception of mechanism must reign supreme.

We are all impressed by the striking contrast between the living and what is recognized as mechanical. The word 'machine' calls before our mind a steam-engine, a spinning-jenny, or a printing press; a gross clattering mass of metal, between which and a rose or a violet the difference seems to be world-wide. The machine obeys laws clearly seen to be mechanical, it is comparatively simple, it appears adapted to the attainment of a particular end, but is incapable of attaining it by any but the one direct path along which we have set it moving. The plant presents the phenomena of life; which means the direct opposite of all this. Into the indefinite complexity of its structure we have no means of seeing clearly; its growth and

development cannot be shown to be the result of mechanical causes exclusively; it appears to move toward an end of its own, and to have a capacity for attaining this end by certain by-paths when for some reason the direct road is obstructed. The plant develops according to a certain plan, and after this plan reproduces its kind. When the end of a branch is pruned away, buds form and new sprouts make their appearance to carry out the idea with which the mutilation interfered. If we have here a machine, it is at least a machine which must not be brought down to the level of the mechanisms constructed by man to carry out his purposes. And if we pass from plant to animal the contrast is, if possible, more striking. I have said above that, in the mechanical view of the material world, the boy who strikes a dog with a stick, and the dog that receives the blow, are simply masses of matter undergoing certain changes in their space-relations to one another, all of which changes are explicable by the laws of mechanics, and form an inevitable succession of states related to each other as cause and effect. Yet the fact remains that a boy whom we recognize to be of a certain stamp will, as we know before the act, hit the dog under the most varying circumstances—whether the animal be on this side of him or on that, within easy reach of him or further away, standing still or moving. He will even chase him around the house again and again; in which case the description of the successive positions of the material particles which make up boy, stick and dog, in their relations to each other and to other things, must attain to enormous complexity. The one certain thing, in the present incomplete state of our knowledge, seems to be that the boy will hit the dog—*i. e.*, that, to speak mechanically, a certain final collocation of material particles will be attained. The path by which it is to be attained seems highly uncertain.

If, then, this boy and this dog are machines, they certainly differ widely from the machines which are commonly recognized as such, and it is manifestly an error to overlook the difference. It is possible to be so impressed by it as to maintain that the notion of mechanism must be abandoned altogether when one is considering such things, and with it abandoned the explana-

tion by a reference to efficient causes which is the very sheet-anchor of science. On the other hand, one may estimate this difference at its full value, and nevertheless believe that the phenomena presented by living beings, growth, development, reproduction, activities of the most varied description, dissolution,—all would be capable of description in mechanical terms, were our knowledge and our intellectual powers sufficiently advanced. One may point out that the possibility of a detailed description of the processes by means of which things come about is not in the least incompatible with the recognition of the fact that such and such things do come about. In other words, one may point out that the existence of *efficient* causes—the 'necessary antecedents' of which I have spoken above—is not incompatible with that of *final* causes, for these latter are only the ends which are attained through the instrumentality of the former.

It is a matter of common experience that it is quite possible to have a knowledge that such and such an occurrence will take place, and yet to be in the dark as to the series of causes which will bring it about. One may know that it is likely to rain, and yet have the vaguest possible notion of those atmospheric changes which give birth to the falling drops. Similarly, the simultaneous appearance of boy and dog within one's horizon may give rise to the conviction that sooner or later these two masses of matter will stand in the definite mutual relation referred to above; and yet one may have no clear idea of the particular series of changes which will precede this particular result. Thus one may know empirically that with one's gun at a certain elevation, with a given charge of powder, and with a given projectile, one may hit a target at a fixed distance. At the same time one may be quite unable to calculate the path of the projectile from the gun to the target. When one knows something of the science of mechanics, one no longer thinks of the beginning and end of this series of changes as constituting all that is worthy of attention in the occurrence as a whole. There are no longer one cause and one effect; there is an indefinite series of causes each followed by its effect, and the initial antecedent is no more important to the final result than

are any of the others. Those who incline to view the universe of matter as a perfect mechanism must look upon the series of changes which take place in the relative positions of the boy and the dog as constituting such a chain of causes and effects. They cannot admit for a moment that the end is fixed independently of the means. To them the end is simply one term in a complicated series, and its coming into existence is conditioned upon the links in the chain preceding it. But they may freely admit that they are sometimes pretty sure of the end when they are by no means clear as to the exact path by which it will be attained, as has been said above. They may point out that we can be very sure when we drop a ball inside of the rim of a bowl on the table before us that the ball will ultimately come to rest at the very bottom of the bowl, and yet we may find it difficult or even impossible to describe in detail all the motions of the ball before it comes to rest. Which means that in a causal series admittedly mechanical it may be possible to predict the appearance of a given term, even when we have no definite knowledge of those that precede it.

To all this it may be objected that it is easy to suggest that all the changes which take place in those masses of matter that we call living beings may find their explanation within the realm of mechanics, but it is another thing to prove that they actually do this. When the boy's gaze has once rested upon the dog, the end seems to be fixed, as in the ancient conceptions of fate, and the means appear to be conditioned by the end, not the end by the means. Can a mechanism select this and reject that, taking what serves a given end and refusing what does not? Has anyone the least conception of a mechanism that can pick and choose in this way? If not, why insist that living beings must be brought under the conception of mechanism?

To this one may answer that, even in the gross mechanisms constructed by man, we are not without some suggestion of selection. To get the bit of chocolate out of the metal case that stands against the wall in the railway station, one must drop the appropriate coin into the slot, just as one must deposit the appropriate coin in order to obtain a sandwich from the

woman at the lunch-counter. And one wholly ignorant of the extent to which the construction of mechanisms has been carried, might easily be tempted to think that the motions of the machine that tests the weight of the coins committed to it, sorting out into different heaps the perfect and the imperfect, are determined by the end to be attained and not by a chain of mechanical causes. To one who understands the construction of such mechanisms there is nothing marvellous in the thought that a definite end will be attained as the result of a strictly mechanical series of processes, and that the attainment of other results will be provided against just because of this series of causes. Between the most ingenious of such machines and the boy of whom I have been speaking, there is doubtless an enormous difference, and one which it would be foolish to overlook. But it should not be forgotten that between the human body and organic structures which are less highly developed there are also differences which are sufficiently striking. We are not compelled to pass at a jump from a weighing-machine to a man. There are forms of life that exhibit phenomena which, if they do not serve to bridge the gulf between the organic and the inorganic, at least bring us to the brink with a strong disposition to launch away. The evidences of what we are inclined to recognize as choice, in an unequivocal sense of that word, grow less and less as one descends in the scale, and the approach to mechanism, as we commonly think of it, seems a sufficiently close one. If we elect to believe that all motions in matter cannot be accounted for by a reference to mechanical causes, where shall we make the break? Shall it be between the organic and the inorganic, or shall it be placed somewhere above this point? The question is not an absurd one, for, as the student of the history of philosophy well knows, thoughtful men have not been at one touching the answer that should be given to it. The disciples of Descartes drew the line between man and all that lay below him. This would make the boy of our illustration something more than a mechanism, but the dog, who appears equally active, and almost equally ingenious, would be a mechanism and nothing more. Modern science, imbued as it is with a strong desire to remove what

seem to be breaks in the orderly development of nature, would find it difficult, having gone as far as this, not to go farther.

The adherent of the view that the material world is through and through a mechanism may argue that the objection which has been urged to his view is, in so far as it really is an objection, nothing more than an *argumentum ad ignorantiam*. If it be merely intended to point out that, on the slender basis of actual knowledge which we at present possess, modesty is an appropriate virtue, and dogmatism a thing to be deplored, even the most enthusiastic student of science should welcome the admonition. It is foolish to maintain that we know, where we only have hints and guesses. It is, of course, also foolish to reject those hints and guesses, if they are the best that we have at the present moment. One should take them at what they are worth, holding one's opinion tentatively, and striving neither to be blinded to new light by ancient prejudices, nor carried off of one's feet by the currents of contemporary thought, which may or may not happen to be setting in the direction of true progress. If, again, the objector merely wishes to emphasize the fact that boys are not such machines as we place in position against the wall of a railway station, and to insist upon the truth that there is in our experience such a thing as the choice of ends and the adjustment of means to their attainment, no sensible man can have any quarrel with him for this. There can be no more serious error than to suppose that because all the changes which take place in a boy's body, and in its relations to other things, can be brought under the conception of mechanism, therefore, the boy must no longer be regarded as a boy, but rather as a bit of furniture. As well argue that because a boy is an animal we must look upon him as a flea. When things widely diverse are brought under the same general concept, it does not mean that the differences that distinguish them are obliterated. It is, therefore, of the utmost importance to remember that an extension of the concept of mechanism does not in the least wipe out the distinction between what are commonly recognized as machines, and living organisms. That distinction is a marked one, and one must be a slave to one's idea when one is misled into overlooking it. To call attention to the distinction,

where there is danger that it may be forgotten, is a public service.

But if the objector does not intend to do either of the things mentioned just above, and does intend dogmatically to maintain that no extension of our knowledge of boy, dog, stick, and their material environment—not even the knowledge of which at present science dreams and which it recognizes as quite beyond its grasp—would reveal that the series of changes which have taken place are part of a mechanical order of things, he seems to arrogate to himself an authority to which he can lay no just claim. Were he in a position to show that the attainment of such and such ends could not be effected by a series of mechanical causes, his position would be a reasonable one. As he is only in a position to show that no one knows just how it can be, it does not appear very reasonable.

It does not seem, then, that we need be deterred from assuming, as a working hypothesis, at least, that the universe of matter is a perfect mechanism, either by supposed difficulties connected with the concept of mechanism itself, or by the fact that science is not now in a position to prove the justice of all its guesses at the truth. But there is one objection which appears to have more weight. In our common experience of the world, it is an undeniable fact that there are such things as *minds*. It is as fair to ask what these are, and what is their true place in a reasonable scheme of the system of things, as it is to ask any of the questions touching the nature of matter with which the student of physical science occupies himself. For an answer to such questions one can no more turn directly to the crude and undigested experience of the plain man, than one can for an answer to questions concerning the nature of matter. Still, there is a way of approaching such questions. And if it be discovered that a given view of the physical universe is really incompatible with what seems, after critical examination, to be known about minds, it is an argument against that view not to be despised.

PRACTICE AND ITS EFFECTS ON THE PERCEPTION OF ILLUSIONS.

BY PROFESSOR CHARLES H. JUDD,
University of Cincinnati.

The results to be reported in this paper were obtained by two observers in extended series of measurements of the Müller-Lyer illusion. The series of measurements were undertaken for the purpose of determining quantitatively the results of practice on the strength of the illusion. The results here presented do not answer by any means all the questions that arose during the investigation, but the method proved to be so well adapted to the investigation of the effects of practice, and the results are so suggestive because of the light which they throw on the nature of the illusion, that the writer has felt justified in presenting this account of the preliminary experiments already performed, and in drawing some of the inferences which seem to issue from the data gathered.

The method was the well-known method of determining quantitatively the strength of the Müller-Lyer illusion. The figure with the oblique lines turned inward was drawn at the edge of a card, and this was placed over a second card on which was drawn a line of indefinite length having at its uncovered extremity the oblique lines turned outward. The two figures of the illusion were thus obtained in horizontal juxtaposition and in such relation that the overestimated figure on the lower card could be adjusted by the observer until it seemed equal to the upper figure with its underestimated line.

A number of different figures were used and will be designated in the following discussions by the length of the standard line, which is in each case the line in the underestimated figure, and by the angle between the oblique lines. Thus, 'figure 54 mm., 90°', indicates that the standard is 54 mm. in length and

that the oblique lines in the figure form angles of 90 degrees with each other. At no time did the observers have any objective means of determining the true relations of the lines. The length of the adjusted line was carefully marked off at each trial on a slip of paper and filed for measurement. The observations were made in series of from 20 to 200 at a single sitting, the sittings covering periods of one half an hour to two hours. Generally, several series were made on the same day.

One of the observers was the writer. He expected the illusion to grow weaker with practice and at the 500th test and again at the 900th he measured the lines to discover how much further the experiment was to be carried. His periods of work, as will be seen from the table and curves, were somewhat irregular.

The second subject E was entirely ignorant of the character of the results throughout the whole course of the experiment. The only expectation of subject E was that the results would grow somewhat more regular. At no time was there any suspicion that the illusion was changing in strength. Subject E was also much more regular in periods of experimentation and in mode of adjusting the figure. During the first practice series performed by E, the figures were always held in the horizontal position, the underestimated figure was always held on the right-hand side, and the overestimated line was always set too long at the beginning of the experiment and gradually shortened until the desired length was reached.

The two observers represent, accordingly, two different kinds of training. Subject J was trained somewhat more irregularly and with a background of abstract knowledge and expectation accompanying the perceptual practice. The subject E was trained in a very regular mode of operation and in a purely perceptual way, without any accompanying biasing knowledge or expectation.

Before entering upon the practice series both subjects made 25 determinations with each of the figures to be used by him. These results are marked in the figures, 'Illusion before practice.'

We turn now to the detailed results. Subject J began the experiment by making 25 determinations each on figures 54 mm., 90°; 54 mm., 45°; and 68 mm., 90°. He then began continuous experiments with figure 54 mm., 90°. In this practice series the figures were held in a horizontal position and the standard figure was uniformly on the right-hand side, but the mode of adjusting the figure was irregular, sometimes starting with the compared line too long, sometimes starting with it too short. The results are presented in Table I. and in Fig. 1. Each entry in the second column of the table, except in a few cases as indicated in column one, and each point in the figure, is the result of averaging twenty consecutive measurements. One horizontal line in the table, and one vertical line in the figure, indicates a pause of an hour or more during a given day. Two lines indicate the pause of a night. The third column in the table presents the mean variation, and the fourth and fifth columns contain respectively the highest and lowest single determinations in the given group of twenty. Full tables will not be given of the other series. The chief results can be exhibited by the curves, and it may be stated here that the results in regard to variation are fully represented by this first case.

In general it may be said that each period of practice results in some improvement on the part of the observer. The column of mean variation indicates also in general in each series—at least until the subject reaches the maximum of practice—an increasing regularity of perception. One especially interesting phase of the curve is the sudden improvement which showed itself after the pause of two days between measurements 760 and 761. There is no special ground for this sudden change. Nothing parallel to it occurred in the case of the other observer, E. It is probably due to the fact, so well known in practical experience, that a pause is sometimes beneficial in the pursuit of any kind of training.

The most important fact is of course the final result. The illusion disappears after practice. It disappears, not by any process of judgment or any process of indirect correction. The line comes to look differently than it did at first. A most strik-

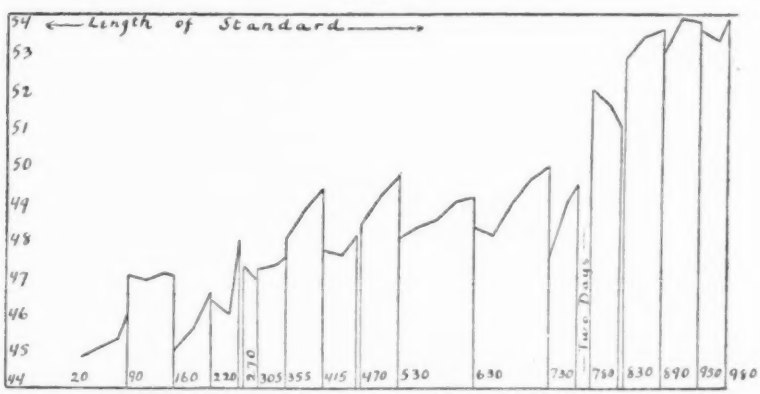
TABLE I.

SUBJECT J.

FIG. 54 MM., 90°. POSITION HORIZONTAL WITH STANDARD ON RIGHT SIDE.

No. of Measurements.	Avg. Illusion.	M. V.	Highest Determinat.	Lowest Deter.
1–20	44.8	0.83	48.0	43.0
21–40	45.1	0.50	46.5	44.0
41–60	45.3	0.87	48.5	43.0
61–70	46.0	0.80	48.5	45.0
71–90	47.0	1.38	49.0	42.5
91–110	46.9	0.79	49.0	45.0
111–130	47.1	0.56	48.0	46.0
131–140	47.1	0.75	49.0	46.0
141–160	45.0	0.88	46.5	43.0
161–180	45.6	0.70	48.0	44.0
181–200	46.6	0.67	48.0	45.5
201–220	46.4	1.03	48.5	44.0
221–240	46.0	1.03	48.0	43.5
241–250	48.0	0.85	49.5	46.5
251–270	47.3	0.95	49.0	45.0
271–285	46.9	1.00	49.5	44.5
286–305	47.2	1.07	49.0	45.0
306–325	47.2	0.85	49.5	46.0
326–335	47.5	0.70	48.5	46.0
336–355	48.0	0.83	49.5	46.0
356–375	48.8	1.07	51.0	46.0
376–395	49.3	0.60	50.0	48.0
396–415	47.7	0.84	49.5	46.5
416–435	47.6	0.84	50.0	45.5
436–450	48.1	0.50	49.5	46.5
451–470	48.4	0.85	50.0	46.0
471–490	49.3	0.81	51.5	48.0
491–510	49.7	0.75	51.0	48.0
511–530	48.1	0.80	49.5	46.0
531–550	48.3	0.77	50.0	46.0
551–570	48.5	0.83	50.0	47.0
571–590	49.0	0.38	50.0	48.0
591–610	49.1	1.04	50.5	47.5
611–630	48.3	0.92	51.5	45.5
631–650	48.1	0.74	50.0	46.0
651–670	48.9	0.80	51.0	47.0
671–690	49.6	0.74	51.0	47.5
691–710	50.0	0.75	51.5	48.0
711–730	47.5	1.18	50.0	45.0
731–750	49.0	0.70	51.0	47.0
751–760	49.4	0.50	50.5	48.5
Two Days' Pause.				
761–780	52.1	0.53	53.0	51.0
781–800	51.6	0.55	53.0	51.0
801–810	51.0	0.20	52.0	50.0
811–830	52.7	0.87	54.5	50.0
831–850	53.4	0.46	54.0	52.5
851–870	53.6	0.49	54.5	53.0
871–890	53.0	0.73	54.5	51.5
891–910	53.9	0.57	55.0	53.0
911–930	53.8	0.81	56.0	52.0
931–950	53.6	0.56	54.5	52.0
951–970	53.2	0.63	54.0	51.5
971–980	53.8	0.39	54.5	53.0

FIG. 1.



ing exhibition of the change which has taken place can be seen by setting the line, after practice is complete, at the length at which the records show that one set it early in the experiment. It seems so strikingly and so clearly too short to the now trained eye that it is almost unbelievable that the illusion could ever have been so strong. We have here, then, a change in the perceptual process, which change has taken place gradually through repeated efforts to deal directly with the object perceived.

One might be tempted to hold that the change was not a purely perceptual change, uninfluenced by expectation, in the case of observer J. It is just at this point that the curve of subject E (Fig. 2) is of interest as a curve of similar import, and, what is more, as a curve of similar length to that of J. Subject E, it will be recalled, had no biasing judgments. The result was in this case due to practice in comparing the lines, and to this practice only.

FIG. 2.

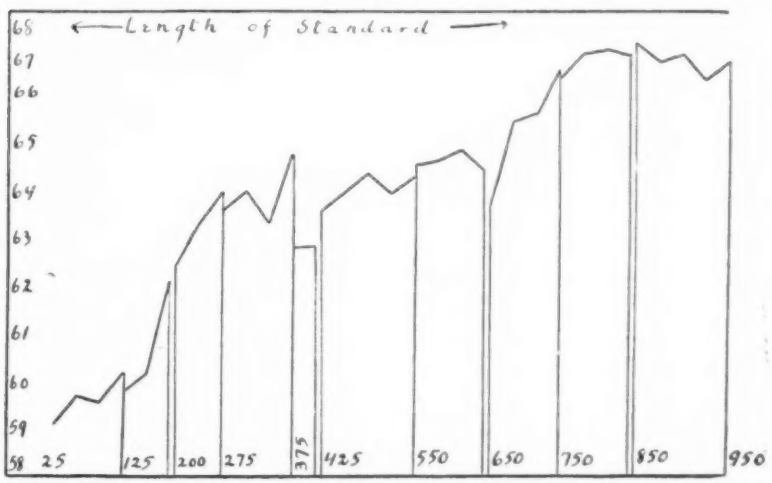


Fig. 2, it should be noted, represents at each point an average of twenty-five measurements. It also shows a more regular development—a fact which is doubtless related to the regularity of E's practice.

Taken together these two series of perceptual changes show clearly that judgment and perception are different kinds of

processes. Judgment cannot do for the observer what must be accomplished by practice. Abstract ideas cannot take the place of direct perceptual experiences. Furthermore, the change which took place in the perceptual process did not reveal itself to introspection. The very mode of conscious activity underwent a change. The observer came to stand on a different level of perception after practice, but the process by which the change of level had been effected can only be inferred; it could not be observed even when the observer was looking forward, as was observer J, to the change.

The significance of these conclusions for the general genetic study of mental life cannot be overlooked. If it is true that in a limited series of experiences a change so marked as this may take place without leaving in direct consciousness a single trace of the process by which the change was effected, then certainly we must hesitate in adopting any explanation of mental development which is based on the assumption that the later stages of mental life are different from the earlier stages only in a quantitative way. The whole genetic problem is seen to be a problem requiring methods which reach beyond introspection, and requiring, furthermore, a careful scrutiny of the qualitative aspects of the changes effected through development.

To return, however, to the experimental results. After the observers had gained their experience with their practice series, a number of additional series were tried. The additional series tried by J may be presented first.

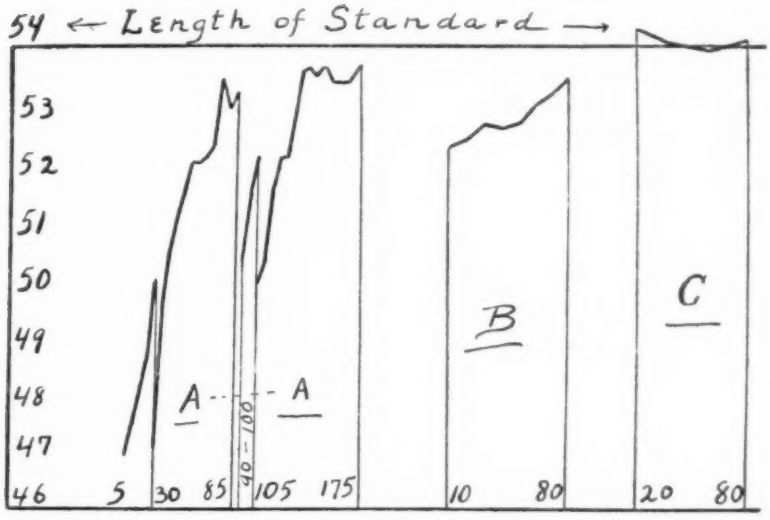
Before practice J had made twenty-five determinations, each with figures 54 mm., 45°; and 68 mm., 90°. The average illusion for figure 54 mm., 45°, before practice was (in terms of the length of the adjusted line) 45.1 mm. with a mean variation of 1.73 mm. After the practice series with 54 mm., 90°, had been completed, twenty-five new determinations were made with 54 mm., 45°. The position, it should be noted, in all these experiments was the same, namely, horizontal, with standard figure on the right-hand side. After practice with the 90° figure, the illusion had nearly disappeared for the 45° figure, though there had been no practice with the latter. The average illusion (again in terms of the adjusted line) was now 53.0

mm., with a mean variation of 1.06 mm. In other words, the effects of practice had been transferred in a very marked degree to the 45° figure, with which there had been no practice.

The results for figure 68 mm., 90°, before and after the practice series with figure 54 mm., 90°, were as follows: before practice 58.7 mm., with a mean variation of 0.95 mm.; after practice 67.3 mm., with a mean variation of 0.95 mm.

If now the character of the figure was changed by turning it around so that the standard line was on the left-hand side, the transfer of practice seemed to be much less direct. Fig. 3,

FIG. 3.

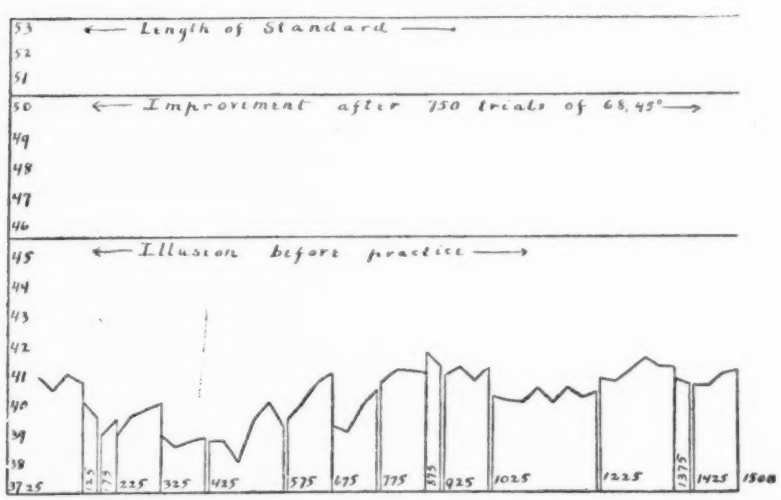


A, shows the results of 175 such determinations with 54 mm., 90°, inverted (each point representing an average of five determinations). This whole series was made after the practice series, but it will be noticed that at the outset the illusion is almost as strong as in the first determinations of the practice series. From this beginning on, however, the curve rises very rapidly, indicating clearly the transfer of practice. Fig. 3, *B*, shows the results obtained by turning the figure 54 mm., 90°, into a vertical position with the standard line above. Similar results were obtained for the vertical position with the standard below. Finally, instead of comparing

the underestimated Müller-Lyer figure with the overestimated figure of this illusion, a comparison was made between the standard 54 mm., 90°, and a simple straight line placed somewhat below and to the left. The result is given in Fig. 3, *C*.

In all these cases there is an obvious transfer of practice, in spite of the varying conditions. In view of the special conditions which seemed to surround the inversion of the figure, as shown in the results reported in Fig. 3, *A*, observer E was especially prepared for a study of that particular phase of the effect of practice. The practice figure of E was, it will be remembered, 67 mm., 45° (see Fig. 2). Before undertaking this practice series E was tested 25 times on figure 53 mm., 90°. This latter figure was, however, in these preliminary and in certain of the latter tests, placed with the standard on the left, instead of on the right-hand side. The result of this preliminary series is represented by the line marked 'before practice,' in Fig. 4. Expressed in numbers the illusion before practice was

FIG. 4.



45.6 mm., with a mean variation of 0.86, the standard figure being 53 mm., 90°.

In order to test E for the easy form of transfer of practice which J had shown when figures of different size were presented

in the familiar position of the practice series, that is, with the standard on the right side, E was given a special series of 25 determinations with figure 53 mm., 90°, with the standard on the right, after completing 750 measurements in the practice series. The result is given in Fig. 4 in the line marked 'Improvement after 750 trials of 68, 45°.' Expressed in numbers the average illusion was at that time 50.4 mm., with a mean variation of 1.3 mm., or an improvement of 4.8 mm.

After the whole practice series was completed, the observer E was given the figure 53 mm., 90°, in the position in which the first 25 determinations had been made, *i. e.*, with standard on the left. It should be recalled that E did not know anything about the disappearance of the illusion in the practice series. No information was given in regard to the change in figure or the difference in position of the standard line. No particular curiosity was aroused by any of these changes in the routine of the experiment. So far as the observer was concerned the problem was merely to set the two lines so that they seemed equal and to make the judgments as regular as possible. Indeed, the subject expressed a preference for figure 53 mm., 90°, because it was shorter and because the oblique lines were further apart, thus making perception and comparison apparently easier. The results of 1,500 measurements are reported in the lower curve in Fig. 4. The mean variations, which are not expressed here, never exceeded 1.27 mm., and were for the most part below 0.90 mm.

The results of this last series are by no means difficult to interpret. The practice which had been gained with figure 67 mm., 45°, with the standard line on the right-hand side, was transferred, but in a negative way, to the new figure 53 mm., 90°, with its standard line placed on the left. That is, in spite of a change in the length of the lines perceived, and in spite of a change in the degree of obliquity of the additional lines, and finally, in spite of a new arrangement of the figures, the effects of practice were obviously carried over to the new conditions. Furthermore, it is evident from the curve that the observer started on this second series of measurements with a thoroughly established habit of interpretation. The failure to improve

during the long series of 1,500 measurements brings this series into the sharpest contrast with the earlier practice series. The mental condition at the beginning of the practice series was one of uncertainty and more or less tentative effort to work out a new mode of interpretation. Gradually there grew up a mode of interpretation suited especially to the conditions presented. Once established, this new mode of interpretation took the place of tentative efforts and became the fixed mode of interpretation. The subject, who had cultivated in a purely empirical way this fixed mode of interpretation, misapplied it, to be sure, and by this misapplication of the mode of interpretation he increased the strength of the illusion under the new conditions. But misapplication though it is, it is nevertheless evidence of transition from an untrained, tentative interpretation to a form of interpretation which is highly developed and firmly fixed.

An interesting contrast appears when we compare E's results with the way in which observer J behaved under similar conditions. With J there is obviously some difficulty in fitting the cultivated mode of interpretation gained in the practice series to the new inverted position of the figure (see Fig. 3, *A*). The acquired habit of interpretation is, however, not carried over in a negative way to the new conditions. It is carried over after some effort and readjustment. This ability of J to accommodate himself to new conditions is undoubtedly a consequence of the less rigid and less purely empirical character of the original practice series. To express the matter in everyday phraseology, J had acquired his mode of interpretation through practice and through practice only, but when he had completed the practice he knew what the character of his acquired mode of interpretation was, and he knew its special character sufficiently to recognize that it must be readjusted in applying it to new conditions. He had in addition to practice what we may call control of his acquired habit.

There are a number of lines of discussion suggested by these results. As pointed out in the opening paragraph of the paper, the experiments have not been carried as far as they may be carried or as far as the writer hopes to carry them, but even in this incomplete state they throw much light on the ques-

tions that have recently been raised in regard to the nature of practice, especially by the papers of Professor Thorndike and Dr. Woodworth.[1] There can be no doubt that in the experiments here reported, a mere change in the length of lines, or even the more marked changes in obliquity of the added lines and in the position of the whole figure, were no hindrance to the transfer of the effects of practice. These changes are, as the writer understands the reports of the two investigators mentioned, fully as marked as many which according to their results hinder the transfer of practice. Perhaps the apparently contradictory conclusions could be reconciled by attention to the fact rendered so obvious by the last series of measurements made by E, namely, the fact that the absence of improvement in a given line of mental activity is often the strongest possible evidence of the transfer of practice. It is evidently quite unjustifiable to judge of the nature of practice by a mere comparison of an observer's results with objective standards. Practice can be discovered only by methods which show something of the course of mental development as a series of subjective and relative changes rather than as a series of objective and absolute improvements.

A second line of legitimate inference from the experiments reported, relates to the nature of the illusion. In the first place, the observer, as he takes up the illusion in the beginning, is obviously without any definite mode of interpretation which he can apply under these conditions. The first stages of his experience with this illusion can not be described merely by saying that he applies to the figure the ordinary mode of interpretation. The fact that change sets in so soon shows that the observer tries a variety of modes of perception tentatively. He depends, of course, in a vague general way on ordinary experience, but he is evidently more or less confused and distracted from the main lines of the figure by the additional lines. The process of development is a kind of progressive perceptual organization in which the distracting lines are mastered and finally interpreted as a part of the larger whole—the final complex process of perception being an advance upon ordinary ex-

[1] PSYCHOLOGICAL REVIEW, Vol. VIII.

perience in that a new special mode of interpretation has been developed to fit the complex group of conditions here presented. When a mode of interpretation has been developed to fit one group of conditions, it shows its special character by not being suited to immediate transfer to other, different complexes of lines.

As already pointed out, introspection does not give a clue to the nature of the changes in the mode of perception. One notes at first that it is natural to try to neglect the oblique lines and concentrate on the main lines of the figure. Early in the practice series both observers noted the feeling of having succeeded in abstracting from the oblique lines. That they had not done so appears in the fact that the illusion continued in almost its full original strength. Later there was less conscious effort directed towards abstracting from the oblique lines; one came to feel a sort of general mastery of the whole figure. This later feeling is the one that came to be more and more clearly marked in the later stages of practice. So far as these observations indicate anything in regard to the later forms of perception, they show that organized perception does not consist in the rejection of factors in the field of vision, but rather in a proper synthesis of all the factors into a single whole. Lower and higher stages of perception are thus to be described, not in terms of the amount or kind of content included in the percepts, but rather in terms of the mode of organizing such content as is present.

An illusion may be defined on this basis as a case of confused perception in which there is no fully developed mode of synthesis, or, as in the case of E's second series, as a perception in which the mode of synthesis is inappropriate to the particular conditions presented.

Finally, a word may be added to the above general discussion in the effort to meet the question which naturally arises as to the nature and conditions of these changes in modes of perceptual synthesis. Here again we have reached one of the limitations of the present investigation. One observation was made in regard to E which may be suggestive. At first the eyes ran over the figures of the illusion in an irregular way.

As the practice series progressed, a perfectly definite habit of eye movement developed. The eyes fixated first the right-hand extremity of the right-hand figure. Then they moved to the left-hand extremity of the right figure which was of course also the right-hand extremity of the left figure. From this second point of fixation a second movement was made to the left-hand extremity of the left figure. Then in one long movement the eyes moved back to their first point of fixation at the right-hand extremity of the right figure. When, now, the observer took up the inverted figure in the second series, the same form of eye movement, consisting of two short movements to the left, followed by one long movement to the right, was regularly repeated. Because of the changed position of the figure, however, the two shorter movements and the subsequent long movement were now, with respect to the oblique lines, in the opposite relation. These facts in regard to E's eye movements were not paralleled by any observable regularity in J's movements.

These facts of movement are the only suggestions which can be offered at present by way of contribution to the explanation of the perceptual changes that take place. But together with the other results they serve to outline the genetic study of the illusion. It is to be hoped that a further elaboration of this method of investigation will give a more definite answer to the questions raised. The purpose of this paper will be accomplished if it serves to suggest a method of investigation and certain preliminary results on the general problem of the development of perceptual processes.

MENTAL IMAGERY OF STUDENTS.

A Summary of the Replies Given to Titchener's Questionary by 118 Juniors in Vassar College.

BY PROFESSOR F. C. FRENCH.

This paper is a summary of the replies given by the class in psychology at Vassar College to the 'Questionary upon Ideational Type' in Titchener's 'Experimental Psychology.'[1] By kind permission of author and publisher I was enabled to have the set of questions reprinted and distributed to the class. This questionary has at least one marked superiority over similar ones that have been used in the past. It offers a fair proportion of tests for all departments of sense. The author very justly says of it: "It is insofar an improvement upon Galton's paper as that the questions upon auditory, tactual, etc., imagery are drawn from definite situations, and not made a mere appendix to the visual portion of the enquiry."[2] While this questionary is doubtless open to criticism, and any individual investigator might suggest improvements from his own point of view, some sacrifice of individual preference will make for the general good. A large number of reports based upon the same set of questions will offer opportunities for statistical comparison that could not be obtained in any other way. It is with the hope that this may be but the first of many similar reports, that I have prepared this summary. I have chosen this particular set of questions not only because of its intrinsic merits, but because it is the questionary offered in the work which is likely to be for a considerable time to come the standard manual of experimental psychology, and it may be hoped, therefore,

[1] Student's Manual, p. 198.

[2] Instructor's Manual, p. 391.

that many other social groups will be tested by the same questions.

The questionary was given out last April to the young women of the Junior class of Vassar College. They were not of course experts in introspective investigation, but they had listened to some discussion of the subject in the lectures, had tried a number of general experiments of this sort in class, and had read the chapter on Imagination in James' Psychology which was the text-book in use. The one thing emphasized in giving out the questions was that the answers should be accurate reports of actual trials. No doubt in the nine or ten thousand answers given there are a considerable number of careless observations and inaccurate statements. But the general average of accuracy is, I believe, very high. Internal evidence fully justifies this belief. When a student answers the question whether she can recall the voice of some well-known person by saying she can for she can always recognize her acquaintances by the voice, we can be sure that she has made no real trial of her power to recall the voice, but is reporting merely what she thinks she can do. The cases that show such direct evidence of having made no actual experiment are very rare—less than a dozen, I should say, altogether. On the other hand the instances that show positive evidence of careful trial are very numerous. Two classes of such evidence are common surprise at failure to recall something, and discrimination in the intensity or vividness of similar images. An example of the first class would be the statement of one who says, "I am greatly surprised that after repeated trials I cannot recall hunger and thirst"; of the second class, "I can recall the taste of salt very distinctly, sugar only faintly and lemon juice not at all." All but five members of the class handed in a set of answers. One of the 119 papers received was discarded because of a marked disparity in age. The writer was a woman who had returned to complete her college course after more than twenty years' absence, and was at least twice the age of the other members of the class. The following results are therefore the summary of 118 replies all written by young women of the usual age of College Juniors.

TABLE SHOWING ACTUAL NUMBER AND PERCENTAGE OF YES AND NO ANSWERS.

	Actual No.		Percentage.	
	Yes.	No.	Yes.	No.
I. Think of a bunch of white rosebuds, lying among fern-leaves in a florist's box.				
a. Are the colors—the creamy white, the green, the shining white—quite distinct and natural?	118	0	100	0
b, 1. Do you see the flowers in a good light?	106	12	90	10
2. Is the image as bright as the objects would be if they lay on the table before you?	59	52	53	47
c, 1. Are the flowers, and leaves, and box, well defined and clear cut?	115	3	97.5	2.5
2. Can you see the whole group of objects together, or is one part distinctly outlined while the others are blurred?	58	43	57	43
d, 1. Can you call up the scent of the rosebuds?	103	15	87	13
2. Of the moist ferns?	65	53	55	45
3. Of the damp pasteboard?	75	43	64	36
e, 1. Can you feel the softness of the rose petals?	114	4	96.6	3.4
2. The roughness of the ferns?	106	12	90	10
3. The stiffness of the box?	106	11	91	9
f. Can you feel the coldness of the buds as you lay them against your cheek?	108	10	92	8
g, 1. Can you feel the prick of a thorn?	85	31	73	27
2. Can you see the drop of blood welling out upon your finger?	110	4	96.5	3.5
3. Can you feel the smart and soreness of the wound?	62	52	54	46
h. Can you call up the taste of				
1. Candied rose leaves?	36	70	34	66
2. Candied violets?	87	29	75	25

3. Salt?	104	14	88	12
4. Sugar?	103	14	88	12
5. Lemon-juice?	104	12	90	10
6. Quinine?	66	40	62	38
II. Think of some person who is well known to you, but whom you have not seen for some little time.				
a, 1. Can you see the features distinctly?	87	31	74	26
2. The outline of the figure?	115	3	97.5	2.5
3. The colors of the clothes?	110	7	94	6
b, 1. Can you hear the person's voice?	86	31	72	28
2. Can you recognize your friends by their voices?	116	0	100	0
3. Can you call up the note of a musical instrument in its appropriate clang-tint: piano, harp, organ, bassoon, flute, trumpet?	106	11	91	9
4. Can you hear in your imagination a note that is too high for you to sing? Think of the playing of an orchestra.	105	12	90	10
5. Can you hear two different instruments playing together?	90	24	79	21
6. More than two?	51	55	48	52
7. Do the tones ring out in their natural loudness?	49	54	48	52
8. Do they come to you from their natural places in the orchestra?	70	27	72	28
c. Can you hear in memory				
1. The beat of rain against the window panes?	113	4	96.6	3.4
2. The crack of a whip?	105	10	91	9
3. A church bell?	108	6	94.7	5.3
4. The hum of bees?	100	15	87	13
5. The clinking of teaspoons in their saucers?	103	11	90	10
6. The slam of a door?	114	2	98.3	1.7
d, 1. Can you see the person in familiar surroundings?	116	2	98.3	1.7

2. Can you see more of these surroundings (*e. g.*, a room) than could be taken in by a single glance of the eyes?	75	26	74	26
3. Can you mentally see more than three faces of a die, or more than one hemisphere of a globe at the same instant of time?	47	70	40	60
e, 1. Do you possess accurate mental pictures of places that you have visited?	113	4	96.6	3.4
2. Do you see the scenes and incidents described in novels and books of travel?	102	14	88	12
f, 1. Are numerals, dates, particular words or phrases, invariably associated in your mind with peculiar mental imagery (diagrams, colors)?	64	54	55	45
2. Are certain sounds always connected with certain colors?	37	81	31	69
3. Have you any other constant associations from different sense-departments?	35	76	32	68
4. Have you a special gift or liking for mental arithmetic or mechanics?	21	93	18	82
5. Can you lay a plane through a cube in such a way that the exposed surface shall be a regular hexagon?	13	103	11	89
6. Through an octahedron?	10	105	8	92
7. Have you ever played chess blindfold?[1]	0	104	0	100
Explain fully how far your procedure in these cases depends on the use of visual images.				
III. Think of the national anthem.				
a. 1. Can you see the words printed?	90	28	77	23
2. Can you hear yourself say or sing them?	102	15	87	13

[1] One student says: "I have never played chess, but can play checkers blindfold. All the figures and the board are clearly defined in my mind."

3. Can you hear a company singing them?	100	7	93	7
4. Can you feel yourself forming the words in your throat and with your lips and tongue?	108	9	92	8
5. Can you hear the organ playing the air?	100	16	86	14
b. 1. Do you recall music easily?	65	52	56	44
2. Do you make up tunes in your head when you are thinking steadily or in reverie?	52	61	46	54
3. Does imagined music take any considerable part in your mental life: *i. e.*, do airs and motives and snatches of music play or sing themselves to you during the various occupations of the day?	81	29	74	26
4. Have you an 'absolute' memory for music: *i. e.*, can you identify a note that is struck upon a piano key-board, or tell the pitch of a creaking door?	14	103	12	88
c. Partly open your mouth, and think of words that contain labials or dentals: 'bubble,' 'toddle,' 'putty,' 'thumping.'				
1. Is the word image distinct?	31	86	26	74
2. Can you think of a number of soldiers marching, without there being any sympathetic movement or movement-feel in your own legs?	36	81	31	69
3. Think of getting up from your seat to close the door; can you feel all the movements?	103	14	88	12
4. As intensively as if they were really made?	32	67	32	68
d. 1. Are you stirred or moved as you think of the words or music of the anthem?	70	43	62	38

2. Are you affected in this way at the theater, or when reading novels?	109	9	92	8
3. Do you choke and cry (or feel like crying) as you read, *e. g.*, of Colonel Newcomes' death?	90	14	87	13
4. When you think of your childish terrors, or of your childhood's injustices, do you feel over again the fear and resentment?	71	43	62	38
e. If you see an accident, the crushing of a limb or the catching of a finger in the door				
1. Do you yourself feel the blow and bruise?	61	49	55	45
2. Does the sight make you shiver, give you 'goose-flesh?'	99	13	88	12
3. Do you pant or hold your breath as you watch a difficult feat of climbing or trapeze-work?	99	14	88	12
4. Can you in general call up organic sensations; hunger, thirst, fatigue, feverishness, drowsiness, the stuffiness of a bad cold?	80	36	69	31

IV. Arrange the following twenty experiences in groups according to the clearness, vividness and distinctness with which you can remember or imagine them.

a. A gloomy, cloudy sky; a sheet of yellow paper; a black circle on a white ground.

b. The feel of velvet; of dough; of a crisp dead leaf.

c. The smell of tar; of a fur coat; of an oil lamp just blown out.

d. The taste of chocolate; of olives; of pastry.

e. The warmth of a hot water bag at your feet; the cold of a piercing wind that cuts through your clothing.

f. Singing in the ear; the buzz of an induction coil vibrator; the preliminary a' of the violin.

g. Nausea; toothache; pins and needles.

V. Give any supplementary information that occurs to you on the topics of this questionary.

1. Do you recollect what your powers of visualizing, etc., were in childhood?				
2. Have they varied much within your recollection?	46	32	59	41
3. What difference do you find between a very vivid mental picture called up in the dark and a real scene?				
4. Have you ever mistaken a mental image for a reality when in health and wide awake?	23	82	22	78
5. Are the characteristics of your mental imagery repeated in the other members of your family?	13	8	62	38
6. Have you a good command of your images?	73	9	89	11

VISUAL.

All answer the first question in the affirmative. Two partial exceptions, however, should be noted. One says the colors are distinct, but not natural, the other that she cannot image the green in a bunch of flowers. In neither of these cases is there any lack of a general ability to image colors as is shown by their answers to other questions. While in answering No. IV. only a few give the image of the yellow paper unmistakably the first place, no one places it among those that cannot be imagined at all. Two other questions involve color imagery: the drop of blood on the finger [I., *g*, 2], and the colors of the well-known person's clothes [II., *a*, 3]. Only four fail to recall the drop of blood, and seven the colors of the clothes.

One of these four is among the seven, but as she sees the color of the flowers and the yellow paper, the negative answers in these two cases must be due to special circumstances, and do not indicate an inability to form color images. In spite then of a few negative answers to special questions, we find no one of the 118 lacking in images of color.[1]

As to illumination there is no such unanimity. While all but twelve see the box of flowers in a good light, nearly half (52 out of 111 who answer this question) say that the image does not seem as bright as the real object.

The imagination of definite forms is as general as that of color. Only three do not have the flowers, leaves, or box, well defined and clear cut. And three again, but not the same three, do not see the outlines of the friend's figure. All see the black circle on a white ground.[2] While, as just mentioned, only three fail to recall the outlines of the friend's figure, 31 cannot recall the features. A considerable number find that the features of intimate friends (parents, brothers, sisters, etc.), cannot be recalled as clearly as those of mere acquaintances or persons seen only a few times. One student expresses a great feeling of relief at finding that this inability to recall her parent's features, which had often troubled her, did not imply any lack of affectionate sensibilities on her part. Forty-three per cent. do not see the entire group of objects together with equal distinctness (I., *c*, 2). Galton reports exactly the same percentage of Charter House boys who have the field of view decidedly contracted. The question as to the ability to see more than three faces of a die or one-half a globe at once is answered in the negative by 60 per cent. Armstrong reports 69 per cent. of the answers to the same question as negative or doubtful.

Accurate mental pictures of places visited are almost universal. Four only answer this question in the negative. Fourteen, however, do not see the scenes and incidents described in novels. This seems to indicate a somewhat lower percentage

[1] Armstrong (PSYCH. REV., 1894, p. 508) reports 9 per cent. of young men students who do not have color imagery distinct and natural.

[2] There seems to be no reason in these cases to justify the statement of Galton (*Mind*, 1880, p. 313) that color is more easily recalled than form.

of imagination images than of memory images. Of the 90 who see the printed words of the National Anthem, a considerable number recall only the first few lines or even the first few words only, while 28 do not see the words at all. Few seem to have vivid visual images of printed words, while many retain the general look of the page and of the part of the page on which anything occurs. Two or three, however, say that they find it necessary to fix the visual images of the words in mind in order to learn anything verbatim. One visualizes words so vividly that she can spell backwards as easily as forwards.

Sixty-four report some sort of mental images habitually associated with numerals, dates or particular words and phrases. The greater number of these associated images are of a very simple character—a column of printed figures with numerals; some picture in a history book with a date. For example, quite a number say that 1492 always brings up a picture of Columbus landing. The following are some of the associated images mentioned: Date 1066 calls up shape of southern part of England; even numbers blacker than odd ones; certain names associated with color; the centuries located in different spots on the map of Europe; word 'fomentation' suggests cream-white; dates associated with colors; months are colored; names of persons and months associated with colors; peculiar words associated with circumstances of first hearing; numerals from 1 to 9 seen as they appear on dominoes, other numerals seen as figures. Various symbolic diagrams for the arrangement of the months in the year and the days in the week are mentioned, most of which seem to have been derived from calendars. As to the variation in the power of visualizing since childhood, the answers are for the most part very hesitating. Thirty-two think there has been no marked change within their recollection, 25 think there has been a development or increase in their ability to visualize, and 21 think that on the whole there has been a decrease in this capacity. Several report an increase in the power to visualize since entering college, due to the training given by the department of English incident to the work in composition writing.

The differences between a vivid mental picture called up in the dark and a real scene are found to be quite imperceptible by 21, five even find that the mental picture is more vivid, stronger, or in some way superior to the real perception. Fifty-five report the image in some respect less perfect than the perception. There is a large variety of ways mentioned in which the picture differs from the real scene. One says the image fades and brightens rhythmically. Several say that the mental picture is smaller than the real scene. Miss F. writes: "A vivid mental picture called up in the dark and a real scene are identical as far as the scene proper is concerned; but in the former case, the scene is seen thrown into an infinite black frame, frame *not* background." Other statements are as follows: In a mental picture each object is called up separately, in a real scene the general idea of the whole is gotten at once; the outline of the image is sharper than in the real scene; the image seems darker than in the real; the image is the same as the reality except as it requires effort to see it; the image is smaller and the details less vivid; the mental picture lacks color and life; as vivid as the real scene but seems infinitely smaller; image has no sound; image seems unreal and off in the distance; mental picture called up in the dark appears as in black and white; figures smaller, puppet-like; image has only the things of interest in a clear light; people in mental picture do not move and seldom speak; the difference is in a kind of unnatural chiaroscuro; the mental image never lasts long, and having a tendency to change requires a conscious effort to keep it before the mind; no distinguishing marks, but does not seem real; all the objects seem darker although they are just as distinct. We may conclude fairly that all of the class are good visualizers, but there are some whose mental images of other kinds are more vivid. Of 84 replies to No. IV. which indicate the precedence of one class over the others, 20 give an image of some other class than the visual the first place.

Auditory.

The tests of auditory imagery in the questionary are very complete. Pretty much every phase of sound is represented—tones, noises, voices, music, timbre, pitch. It is interesting to

note that while there are none who cannot recognize a friend by the voice, 31 out of 117 cannot recall the voice of the person imaged. Certainly recognition does not depend upon the revival of memory images, as some psychologists seem to teach. Some report that they cannot hear the note too high for them to sing unless they imagine some one else singing it. This indicates an inability to separate the sound proper and the motor sensations in imagining their own voices. A similar close connection between motor and auditory vocal images seems to be implied in the case of one student, who says she can identify the notes of the piano, but only within the compass of her own voice. That only 15 report that they are unable to hear themselves say or sing the words of the national anthem arouses the suspicion that some may have mistaken the motor images of the words for the true auditory.[1] But this suspicion is perhaps unwarranted, for there are only 12 who fail to recall the note too high to sing, and all can recall at least some of the noises. Two of the 15 are among the 12, and 9 of the 15 are among the 52 who do not recall music easily. Comparing all the answers to the various tests of sound images, I find that no one in the class is lacking in the images of either noises or tones. In arranging images according to their vividness, 3 put auditory images in the first place and 12 in the second place. But for the specially difficult character of the examples suggested (IV., *f*) their number would, I think, have been much larger.

[1] My own inability to experience vocal imagery of any but the motor type has perhaps made me too suspicious on this point. Baldwin says, "I cannot recall the words of a song until I get the tune" ('Mental Development,' p. 442), and again, "Internal tune is almost entirely auditory" (p. 439). My own introspection finds exactly the reverse. Internal tune is almost (if not quite) entirely motor, and I cannot recall the tune until I get the words or the motor sensation of at least inarticulate humming. I have never been able to discover any distinct and separate auditory images. If I have any auditory imagery at all it is as a shadow accompanying the more substantive motor images. That the motor images of my voice have this auditory tint or shadow seems borne out by the fact that imagined music excites in me in a faint way the same emotional effect as music actually heard. I feel, therefore, that I must plead an exception to the rule announced by Lay (PSYCHOLOGICAL REVIEW, Monograph Supplement 7, 1898) that nobody except the born blind and deaf would deny the existence of visual and auditory images. My hearing is ordinarily good, but as far as my own experience goes I should deny the existence of auditory imagery. Of course I do not doubt the existence of auditory images in others.

Nearly one-third report constant associations of sounds with colors. There is one quite numerous group of these associations which show a close similarity. Examples of this group are the following: The higher the sound the lighter the color, and *vice versa;* loud sounds dark, high sharp sounds light; low sounds dark, high white or yellow; high harsh sounds associated with bright gaudy colors like red or yellow, softer gentler ones with lavender and blue; high shrill sounds with cold colors, gray and blue, lower fuller ones with red, rose and bright warm colors; thin rasping sounds suggest lighter tints, blues and greens, deeper fuller tones suggest reds; high notes always bright hard blue color; high notes associated with the light colors and brightly lighted objects, low sounds with soft lights and dull or dark colors. A few of the more or less miscellaneous associations of sounds and colors may be mentioned. A thin high voice, white; deep voice, green; thunder, black or gray; beautiful music calls up beautiful scenes; sounds of violin bring up blue, of drum red; a harp sounds yellow; a trumpet blast red, note of piano blue; the sound of a certain voice is white; certain laughs always seem purple.

Tactile.

One member of the class answers the questions as to feeling the softness of the rose petals, the roughness of the ferns, the stiffness of the box and the prick of the thorn all in the negative. She remarks that she gets 'the textural effect by sight alone.' She does, however, recall very faintly the feeling of velvet, dough and a crisp dead leaf. This is the case that comes nearest to having no tactile imagery. Four find their tactile images the most vivid of any, and 23 put images of this class in the second place. Some mention associations of tactile and color imagery, *e. g.*, everything soft feels gray, many harsh goods yellow; colors feel differently to the hand, red feels rough.

Gustatory.

Two only answer all the tests of taste in the negative. One of these remarks that she cannot recall taste but has accompany-

ing musclar sensations.[1] Two again put the taste images first in order of vividness and seven put them second. One tells of 'the immense amount of pleasure and satisfaction' she received from eating imaginary ginger-cookies at a time when her appetite had returned after she had had typhoid fever and when she was not yet allowed to take anything but broth. Another says she can call up scarcely any recollections of taste and smell except unpleasant ones, 'for I have a very heavy cold.' Associations of taste and color are mentioned; *e. g.*, one says tastes usually bring up certain colors; another, onions taste green; certain tastes are pink, others green, and one flavor is brown. One tried with success the experiment of tasting in memory one food while eating another. "I found," she says "that I could taste things with a strong flavor (the best results were obtained by a salad dressing of olive oil and vinegar in which the vinegar was very strong), but that while so doing I could not taste the food I was in reality eating."

Olfactory.

Two are unable to imagine any of the examples of odor suggested, and one of these two is also one of those who have no taste images. Four put an image of smell in the first rank in order of vividness, eight in the second place.[2]

Thermal.

One student answers all the tests of heat and cold images in the negative. Two, on the other hand, put temperature images in the first rank, and 10 in the second rank. There are some associations of temperature with color mentioned. One says certain colors are always associated with temperature; another that red suggests a loud noise and heat, green feels cool, another too associates red with heat.

[1] This is exactly my own experience.

[2] Cf. Wundt ('Human and Animal Psychology,' p. 286): "It is in most cases illusion when you think you can recall the scent of a rose."

Bentley (*American Journal of Psychology*, October, 1899), "Images of taste and smell are comparatively rare."

On the other hand Dr. Gamble says fully half of her 65 Wellesley subjects had smell images (Titchener's Experimental Psychology, Instructor's Manual, p. 393).

Motor.

There are nine who report that they do not feel the words of the anthem in the throat or with the lips or tongue. Two of these nine can think of the labials and dentals distinctly with the mouth open, and three including these two do not have the sympathetic movements when thinking of soldiers marching, and also belong to the 14 who cannot feel the movements of getting up and closing the door. Several report that they have the feeling of movements in sympathy with the marching soldiers only if they imagine military music at the same time. As far as we can judge from the few tests offered, there are two or three who lack motor imagery. Among the twenty experiences to be arranged according to clearness and vividness there are no motor images suggested.

Pain.

Fifty-two do not feel the smart and soreness from the imagined prick of a thorn. Seven cannot recall the toothache, three of whom are among those who do not feel the pain of a thorn prick. Closely allied to the pain images are the sympathetic feelings excited by witnessing an injury done to another (see III., *e*). All but 13 report some sort of feelings of this kind. Some say, however, that instead of feeling the blow and bruise, shivering and having 'goose flesh,' they feel faint or sick. One reports that in her mind pain has shape—*e. g.*, stomach-ache is hexagonal.

Organic.

Eighty report that they can call up organic sensations in general, though many of these are unable to recall some of those suggested. Several note that they cannot recall any image of organic sensations unless at the time they are inclined to have that sensation.

Emotions.

Seventy-one out of 114 report that they can feel over again fear or resentment experienced in childhood. One of two who do not recall fear or resentment, recall embarrassment. That so large a percentage can recall childhood's emotions, goes far to justify those who maintain that affective states can be revived. Still

this does not by any means settle the question. The distinction between a revived resentment and a present sympathetic feeling of resentment at the cruel treatment of the little child imagined in mind, must be slight at best, and it seems to me quite possible that even as many as 71 observers may have mistaken the latter for the former. Very few answered the question as to the similarity of their own imagery and that of the other members of their family, and those who did answer it expressed so much uncertainty that little weight can be given to their replies. As far as they go, however, they indicate some tendency on the part of mental imagery to repeat itself in the several members of the same family.

Hallucinations were reported by 22 per cent. Most of these are extremely simple, *e. g.*, one thinks she hears her name spoken and turns to find no one else in the room; another sees a handkerchief on the floor, stoops to pick it up and finds nothing there. There are several fairly good ghost stories told, however, but these are all isolated experiences except one. This case is described in the words of the subject as follows: "I have one mental image that returns to me constantly, and has for more than a year. If I fall asleep on my left side, I awake almost instantly and see some one standing by my bed. The figure is always the same, and I see it until I close my eyes or get up and light the gas. No one else in the family has ever had such a mental image."

Considering the papers as a whole, I should say that the differences in mental imagery of the several members of the class are almost entirely a matter of degree. All are able to call up visual, auditory and tactile images. Only one or two in each case are lacking in either taste, smell, temperature or motor images.[1] This almost universal capacity for all kinds of sense imagery is due, I think, to two causes. First, the subjects are young women. Just how far this cause is operative can be told only when an equal number of young men are tried by the

[1] Stetson (PSYCHOLOGICAL REVIEW, 1896, p. 403) says of 100 Oberlin students, "None lacked visual or auditory images though one considered the auditory doubtful. One lacked motor images, three lacked tactual images, some four were without much imagery."

same set of questions. Second, there is a relatively large number of tests offered for each class of images. Those who could not recall one of the examples suggested, could some of the others. If one were to generalize from this single set of answers, he would conclude that in most people the mind is capable by effort of all kinds of sense imagery, although as a usual thing its content is limited to one or two special forms.

DISCUSSION AND REPORTS.

NOTES ON SOCIAL PSYCHOLOGY AND OTHER THINGS.[1]

I. The Socionomic and the Social.

The criticisms of this book have made it plain—what usually occurs, indeed, when a large problem is approached from a restricted point of view—that further explanations are needed as to the presuppositions of the text. Dr. Bosanquet criticises it from the point of view of the philosophy of the state (or of society). But philosophy of society is one of the 'other things'—it is not social psychology. Dr. Bosanquet's complaint is that invention and imitation are not two things, factors, 'moments,' philosophically considered, but that they are only aspects of a single principle, 'identity in change'; and Mr. Ball follows Dr. Bosanquet. Philosophically this may be true. I, indeed, find Dr. Bosanquet's own views, in his work 'Philosophical Theory of the State,' in the main satisfying. I had myself indicated that my views might go very well with an idealism in social philosophy of the type held by Hegelians; but as a worker in science, in genetic science, where facts, oppositions, dualisms, and pluralisms of all sorts, are the material, his formula is the purest tautology. 'What doth it profit' the sociologist, the statistican, the reformer, the observer of this invention—say, the cotton-gin which transforms a great branch of industry—or of that imitation—say, a lynching party following a leader—'to gain the whole world'—the 'general will' which both may illustrate as identity in difference—and lose the soul—the concrete

[1] These notes are matter added in the forthcoming third edition of the writer's work 'Social and Ethical Interpretations.' Their form and certain allusions are explained by that fact. The following criticisms of the book are those referred to in what follows.

Criticisms of Earlier Editions of this Work.

J. Dewey, *The Philosophical Review*, July, 1898; and *The New World*, September, 1898. J. H. Tufts, The Psychological Review, May, 1898. H. Havard, *Revue de Métaph. et de Morale*, Jan., 1899. F. H. Giddings, *Science*, Jan. 6, 1899; and in 'Democracy and Empire.' S. Ball, *Mind*, April, 1901. W. Caldwell, *American Journal of Sociology*, Sept., 1899. C. A. Ellwood, *American Journal of Sociology*, May, 1901. B. Bosanquet, *Mind*, May, 1901, and in 'Philosophical Theory of the State.' P. Barth, 'Einleitung' to the German translation of this work (Leipzig, Barth, 1900).

social somewhat which distinguishes the two cases! Go to the biologist in the analogous case and speak thusly: "Cease correlating and measuring variations, and cease figuring out hereditary likenesses: the principle of life is the principle of identity in change." He will reply: "Indeed, quite possibly." But his work will go on, and he may say further: "It was not on your formula that the modern evolution theory was established, nor by it did Darwin discover natural selection; but the rather by observing variations as such, and cases of its opposed principle, hereditary resemblance." I have said as much in the section on Hegel's views[1]: what is wanting, to bring science to the support of philosophy, is a formulation of the actual uniformities and oppositions, and the discovery of the processes by which these occur. This is the business of social psychology, on the one side, and of the social sciences on the other. Social philosophy is 'another thing.'

Then there is biology, and with it individual psychology, as such. These, too, are 'other things.' It is interesting to find the distinction made in Sect. 313*a*—between social forces as such, and the limiting and directing conditions under which they work—and covered under the terms 'socionomic' and 'social,' recognized by Comte (cf. Barth, *Philosophie der Geschichte als Sociologie*, I., p. 33 f.). The neglect of it since Comte is remarkable. My critic, Professor Ellwood, fails to observe it, and so charges me with neglect of these 'other things.' I am not second to anybody in the recognition of the biological forces —of natural, artificial and sexual selection, of struggle for existence, of competition of types and of group selection—as conditioning and directing social evolution. But my work is to investigate the *social*, not the socionomic: the forces implicit in the social movement—the uniformities, oppositions, and processes of social change. These are always inside the social groups, not between social groups; if between them, then by this very fact they become parts of a larger group within whose movement the social forces are immanent. These 'other things' belong to the sociologist, who aims to discover all the conditions as well as all the properly social forces of social history; but not to the social psychologist. And even then it is his business to recognize fully in the spirit of Comte's distinction—what he never does!—these

[1] It may be noted that I have gone so far, in a footnote, as to say that the philosophical supposition of a 'real or general self'—that is a truer way of speaking, than of a 'general will'; truer to the facts I mean—implicit in the whole process of social organization is at least not excluded by my 'self-thought' theory. *Qua* philosopher, one might say more!—but only in a philosophical context.

socionomic forces as outside the truly social. The biologist often falls into the same confusion, calling the geographical environment and natural selection biological forces; but as soon as we substitute 'vital' for 'biological,' we see his error. Professor Ellwood's criticism on this point, therefore, not only fails to reach home; it illustrates what is to my mind a common and glaring confusion of thought (unless, indeed, it is in the interest of general sociology that he writes; for in that case, apart from details, I, *qua* sociologist, accept most that he says).[1] It is just this sort of confusion of things and 'other things' that makes this whole subject the pseudo-science that it is to-day in the eyes of many.

The same—to come closer home—is to be said as to the relation of individual psychology to social psychology. Only these mental states and processes which are 'social' as now defined belong to social psychology; only those which are, actually *are* elements—not merely condition, limit, advance, hinder states that are elements—in a whole which implicates more than the strictly private life of the one person that has them—only these! A distinction is marked by the terms 'autonomic' or private, and 'socionomic' (public, social) in my note in Groos' *Play of Man* (Eng. trans.), p. 4. Groos divides the "impulses by which the individual wins supremacy over his own organism without regard to other individuals * * *" from "such other impulses as are directly concerned with his relations to others." Not inquiring whether what is strictly private or autonomic actually exists, we may say that a large part of the individual mental life is at least socionomic (just as we say above that the biological very often is); but now we ask further: what part of this is truly social in the narrow sense of being intrinsic and essential to a social, and to every social, situation? This alone concerns us for our problem, although one might define social psychology more widely as including the socionomic in general. So much said—Professor Ellwood brings the charge that I do not allow for various of these socionomic mental processes by which the social life is conditioned and its direction determined (as, for example, the impulses of rivalry, acquisition, sex, the emotion of fear, etc.). Of these, I have to say that they are real and powerful things, and to them the social life often owes its direction, its variations of character, its forms of operation, and much beside. A writer on sociology must be true to psychology on all these things, and much of my book, as

[1] The same applies to the criticism of Professor Giddings, which I find, however, difficult to deal with on account of the crude and ill-considered psychology. See below.

Professor Ellwood truly says, is devoted to them (Part III., 'The Person's Equipment'). Why then—he goes on to ask—is the psychological factor in social organization limited to one impulse, 'imitation,' and to one form of mental content, 'thought'? The answer is that the doctrine does not disregard the others—the 'other things'—of psychology; but it finds them socionomic only, not intrinsically social. Sometimes they are there in a social change, many or few of them; sometimes they are not; what is always there, the psychological thing which is essential and sufficient, is the sort of thought which I call 'self thought,' and the mode of its growth and propagation, imitation. Given these, social life is possible; but there still remains the relative determination of it by the 'other things,' to be worked out by the sociologist. This is my view; and such is the reason that the true and powerful factors which my critic dwells upon are not made more of in a work on Social Psychology.[1]

To say, as Professor Ellwood does, that such a process could go on in a vacuum is—respectfully submitted!—nonsense. The banks are not the river, but where is the river-course without banks? Chemical processes are not of themselves vital, but where is life without H_2O? Similarly where organic evolution without the bionomic?—and this quite apart from the theory of imitation which Professor Ellwood is mainly criticising (and which, I may say, is not mine).

So here, as elsewhere, there is a gradation, a hierarchy, in science: chemistry necessary to life, but not itself of life; forces in the environment necessary to evolution, but not themselves vital; life processes necessary to consciousness, but not themselves mental; consciousness necessary to society, but not all consciousness social; social consciousness necessary to social organization, but not all social consciousness actually in a social organization. In every department of science there is much that is 'nomic' (conditioning, determining, directing), but not intrinsic to it. Whether or not this be accepted as a working distinction in this case, it will nevertheless serve to show what the chapters 'Social Forces,' 'Social Matter and Process,' and 'Social Progress' in this work aim to accomplish, and also to take point from the criticisms that they do not recognize those 'other things' which are really outside their scope and aim.

A further word in this introduction on the general class of topics covered by the term 'sociology' may not be found irrelevant to the

[1] The criticism (by Ellwood and Giddings) that I here make a break in the evolution process is, I think, entirely without force, as I aim to show in a later connection (see below).

main subject-matter of the work. In my view the special social sciences furnish foundations for a general social science, and this latter is sociology. It deals (1) with the nature of the social as such: what is common to the special social sciences—its analytic branch—and (2) with the natural history of societies: the problem of social evolution—its genetic branch. The reader may consult the table given in my *Dictionary of Philosophy and Psychology*, art. 'Social Sciences,' showing the logical and methodological divisions of such a general science. The same state of things appears in biology (a very similar table is given in the same work, art. 'Biological Science,' with the added authority of Professor E. B. Poulton), and the same distinction between 'general' and 'special' is in common use in that department of science.

II. Animal Companies and Human Societies.

It remains to gather up the facts regarding the forms of quasi-social organization among animals. The distinction made between human and animal common life is, of course, not a hard and fast one. The types are respectively types, not kinds. In saying that man is a being whose social life is an organization arising from his growth as a self —as a being who thinks himself and so thinks others also in relation to self—is not to say that there are no factors in his social life due to the lower functions—impulses, emotions, instincts, etc. Man is also an animal. He has certain spontaneous tendencies companywards, apart from his great capacity to think himself into conscious social life. This, however, if it were all he had, would lead to the sort of gregarious life called above 'socionomic'; that is, in the main. That is what the animals have. In its *type* it is a life together, because it is natural for them to live together. It represents the 'instinctive' and 'spontaneous' periods of equipment. This fully admitted—that there is such company life among animals—we yet find it different from the human, just as the child's early spontaneous reactions—bashfulness, organic sympathy, etc.—are different from his later reasonable and reflective attitudes. Yet the transition is gradual, as the springing up of the form of organization called the 'self-thought' situation is gradual. I have endeavored to show the child's progress in actually passing from the lower stages into the higher. So with the animal forms: they are *mainly* instinctive, *somewhat* spontaneous, *a little* reasonable —in the highest species,—*never* ethical. If the individuals of a particular group have a germ of self forming within them, then their organization is becoming tinged with true 'social' value, though in its

type it remains still that of a 'company.' The criticism (Ellwood) that I find here a break in the genetic line—an impassible gulf between animals and man—is contradicted by my whole view of the social life as a gradually developed thing emerging with the consciousness of self. Yet, this continuity of development assumed, the point emphasized in the foregoing pages is the fact of a growing and typical difference between that gregarious consciousness which mainly reflects fixed and unprogressive nervous functions biologically selected, and that consciousness which, becoming freed from these limitations, shows its capacity for the psychological organization which is intellectual and ethical. To this latter alone I apply the term 'society'; to the former, 'company.'[1]

III. The Process of Social Organization: Imitation.

The discussions so far assume a certain definition of imitation, and also a distinction between the function itself and its exhibition in social life. By definition, I understand imitation to be either (1) a process in which one individual uses another as a copy for his own production of something, whether or not he intentionally and consciously aims at the other as his model; or (2) the same type of function when that which is imitated belongs to the imitator himself instead of to another person. The first of these phenomena I propose to call 'social imitation': it is the sort of imitation described mainly by the sociologists (Bagehot and Tarde). Psychologically, the latter is that which is called the 'imitative function,' or 'psychic imitation,' as mainly dealt with by the psychologists (Royce; the present writer, in 'Mental Development').[2] As type of function, this cannot be denied the name imitation, for the process of imitating a copy is precisely the same in the imita-

[1] Criticisms of the view that the social matter is the 'self-thought,' turn largely on the necessity of recognizing the animals' gregarious activities. This we may fully do; but the problem then still remains: how can we get human society with its characteristics?—*i. e.*, (1) reflective opposition to or confirmation of the gregarious impulses; (2) the universality and publicity of social duties and rights; (3) the peculiar 'general' or will self; (4) the institutions in which all these are embodied, notably the state. These things are so outstanding!—'man with his social history is so different from the brute with his physical heredity!'—that the real need is to bring out the human factors, not to obscure them. See, for example, the inadequate outcome of the biological (as opposed to a psychological) naturalism of such a writer as Sutherland (*Origin and Growth of the Moral Instinct*).

[2] The purely neurological self-repeating function which in that work I called 'organic imitation,' is better known as 'circular' process. It underlies, however, in my opinion, all the higher imitative functions.

tor's consciousness, whether the copy arise in his own mind, or be introduced there by another person. But the social phenomenon is social simply and only because there are two or more persons necessary to the imitation, and hence the confusion arising from the failure to discriminate the two points of view. Psychological writers have been careful to mark off the sphere of 'self-imitation' (by this term) from that of 'social imitation.'[1]

In the study of social process, it is clear, we may take the point of view of social psychology—that of the question: by what mental process men actually are social and show social organization? But it is possible also to take the point of view of sociology—that of the question: what do I as an observer find going on between or among men who are socially organized? If one replies to the first question, 'imitation,' he means a different thing from his possible reply, 'imitation,' to the second question. By saying that the social process is imitation, I mean, for example, more than M. Tarde does, who speaks from the objective point of view. In short, the observer sees often what is not 'social imitation' going on about him; he sees opposition, invention, discussion, etc.; and often he sees in imitation less—he sees social imitations which are not productive at all for social organization. But from the point of view of social psychology—of psychic imitation as a function of the individual's life and growth—all of it may still be imitation of this latter type. This is what I believe; it is, indeed, implicit in the foregoing pages, and is now to be brought out more plainly.

We may approach the subject from the point of view of sociology, and ask for the limitations of the sociological theory of 'imitation.' These have been brought out by many recent critics.

First, we are told that much imitation of one by another is not fruitful. This is true (see the criticism of M. Tarde's view, section

[1] In the careful treatment of the terms 'Imitation,' 'Copy,' 'Model,' etc., in my *Dict. of Philosophy*, the topic is brought into line with others cognate to it under the headings 'Mimetism' and 'Resemblance,' where the term 'mimetic resemblance'—that in which what *is resembled* is itself a factor in the production of that *which resembles it*—is made to cover both cases of imitation. The use of the term 'instinct' as applied to imitation is, I think, confusing. As used in my *Mental Development*, it means simply what is a native tendency or impulse, not an instinct in the sense of a function having a fixed form of reaction or expression. I now follow the recommendation of the *Dict. of Philos.*, and call it the imitative impulse, which, I think, is native for the reasons I give in arguing the case in *Mental Development*. Cf. Groos's similar revised usage in this case, and also in that of the play impulse (*Play of Man*, p. 2).

316, 1), but it may still be true that what is fruitful always involves psychic imitation (or even social imitation). This criticism holds only against the view that social imitation is always fruitful for social organization, which I think is far from correct.

Second, we are told that although imitation may be present, it is not that which is fruitful and essential: (1) the recognition of another self, (2) the constraint of obedience enforced by another, (3) the compulsion of ideas, (4) the onward sweep of the social current, (5) the sharing of a 'general will, (6) the recognition of duties and rights, (7) a social contract—all these are urged, and urged by those who criticise the 'imitation' theory.

Again, we may say, this negative criticism, coupled with most varied positive views, holds only against the theory that 'social imitation' is the essential and the only essential thing. But admitting its force, we ask: Are all the things mentioned as real social agencies—or any of them—adequate without psychic imitation, without the exercise of the imitative function in the social individual? And we find that they are not. They all involve a form of social matter which can only have arisen, and can only be operative, in a social situation through the imitative function. We may take them up in order:

(1) The recognition of another self, or of many other selves. This is only possible when and because the self-thought has grown up through direct social imitation with the further use of the same thought by ejection, which is self-imitation. The self arises through the reinstating, by imitation, of a copy found in others, together with the reading back of the enriched self-copy into the others. If the taking over from you to me is imitation, how does the function differ when I carry over from me to you? If this be true to psychology, then the recognition of another self is an imitative function through and through. At any rate this is a position which is not touched by the criticism in question aimed at the 'social imitation' theory.

(2) Constraint and obedience. Here the lesson taught, the task enforced, the obedience required, depends upon one's accepting and acting on what one is told; and acting on what one is told is a form of self-imitation which is only one step removed from direct social imitation. Where is the difference in my function between doing 'what I see you do and doing what I hear you tell me to do'? It is said the motive for the doing is different; and so it is. But it is the entire act which is or is not fruitful for social organization, not merely the motive to it. To be sure, the motive makes a difference; but the motive as such is not the criterion of its social availability. A

whipped dog obeys from fear, and so may a whipped man; but the man's act, motived by his fear, modifies or confirms his social status in his thought and in that of others; the dog's does not.

So I hold that social constraint, all that compels and enforces, in so far as it is social and not merely 'socionomic,' is so through its acceptance and assimilation; and this is then subject to the law of all social material that it be taken up by imitation in the social agent's personal self-thought.[1]

So it is also with the factors mentioned above as (3) and (4)—the compulsion of ideas and the social current. These get in their work as strictly social only through their acceptance and assimilation by the social agent. The valuable data of M. Durkheim's book on *Suicide* can be fairly understood, I think, only on the supposition of a constant psychic imitation whereby the leviathan, society, finds his roarings echoed in numberless cries, the voices of the individuals who are the organs of society—and this in spite of M. Durkheim's strenuous opposition to M. Tarde's imitation theory. Only a social agent can be compelled to be sociable, and only he can be a social agent who is socialized. It may be true that social conditions compel a certain number of suicides a year; but it is also true that each man himself commits suicide—otherwise it is not suicide, but murder. One may say that I am wrong in making this socialization proceed by the one process of self-growth through imitation. It may be. But still this theory is not touched by the criticism which merely points out that social imitation is absent in this case or that.

(5) Into the 'general will'—postulated by still others—in my opinion, psychic imitation enters. To partake of a general will—or a general mind, or a general anything, 'general' meaning in some sense 'collective'—one's private will, self, mind, consciousness, must implicate others in a collective outcome. What is the good of a general will if the individuals do not reflect it? It is just of its essence that they do. But this involves some mental content not only *common to their thought severally, but also thought by them as common.* This is what I mean by 'publicity'; and I hold that this arises in a common imitative situation. A will is not collective simply when n individuals agree in willing this or that. Each must will this or that as collective—as belonging to a public of individuals in the thought situation in which he finds implicated as he himself is. This implication

[1] A similar result appeared in the chapter on 'Sanctions' (Chap. X.), where we found that social sanctions to be effective have to be taken up and ratified by the individual as 'personal' sanctions.

of all in a common situation by the thought of each is, I think, possible only through the imitative development of the self-thought. This, I may say again, may not be true; but the assertion of a general will is entirely incompetent unless one can show how a general will is a psychological possibility and just what its genetic factors are. Rousseau and, more recently, Bosanquet make no effort to do this. The latter criticises imitation loosely, without seeing that this theory utilizes the imitative functions to derive the general will; in criticising me, he is hitting blows on the plate which this theory is placing over an exposed joint in his own harness. Yet in the main I indorse his criticism of the *sociological* imitation theory.

The 'status' theories, which (6) hold to a recognition of duties and rights as the essential thing, and the 'contract' theory, which (7) holds to a social contract, both point out something in the main true, but not analyzed in its lowest terms. How are duties and rights possible?—and how is this or that status possible?—how does man come to give his adhesion to the contract implicit in social organization? These questions I have endeavored to answer by depicting the process whereby the individual, in growing to be a person—by the dialectic of his personal growth—is at once also a social person with a status, and with duties and rights.

So also a social contract—in any sense in which it exists at all—is the individual's ratification of all that the status or social situation means. This, therefore, comes to supplement these partial theories. A genetic theory points out the origin of the developed social life with all its phases; and if my way of doing it be correct, psychic imitation is an essential mental process in it all.

Our result, therefore, made now more clear from this review of criticisms, is that imitation is the method or process of social organization in two senses: (1) Ideas, inventions of all sorts, are actually propagated by the imitation of one man by another; but this is only one step in their conversion into social matter. Merely this fact of social imitation does not necessarily make these things socially available. If so, my parrot would, by imitating me, come into a social status with reference to me. Another factor is necessary, *i. e.* (2) imitative assimilation and growth, whereby what is imitated is organized in the individual's own thought, and imitatively ejected into others, becoming part of a situation—a status-scheme—whose organization includes 'publicity' and 'duties and rights.' It is only this full view, not the first part of it taken alone, that I am concerned to defend.

IV. Selective Thinking.

Mr. Bosanquet brings the positive criticism that I do not develop an exact view of the process of selective accommodation by which 'the mind can appropriate a law or principle, the scheme of a whole, and naturally and necessarily differentiate its reactions in accordance with the bearing of such a principle on the new situation presented' (*Mind*, 1899, p. 174). This is covered, I think, by the points given in the section on 'Selective Thinking' (Sect. 78), with the preceding sections on the nature of invention (Sects. 54–57), and carried out in detail in my later 'President's Address,' printed in the forthcoming volume, the third in this series, entitled 'Development and Evolution.' Briefly, I hold that in each such case the 'scheme of the whole' is itself the outcome of an earlier active accommodation (or of many of them); not only does action result in the selection of thoughts, but thoughts are the counterpart of former adapted actions. So in each case to recognize the ready-formed 'scheme of the whole' is merely to recognize the earlier organization—what in my 'President's Address' I call the 'platform'—by which the thinker is able in so far to 'size up' the new situation. The general process by which all accommodation is effected must go deeper than goes the assumption of a plan having itself no genesis; unless, indeed, we bring in intuition or some other form of 'preëstablished harmony' between thought and things. On my view the whole process is one of these phases: (1) the selection of actions which 'work' in a given situation, (2) the corresponding and consequent survival of the thoughts which are functions of such selected actions, and (3) the 'system of the whole' *so made up* which is brought to new situations; this last is but the mind's progress so far, in this line or that, in the two earlier mentioned phases of its growth. In short, the twofold psychological truth that (*a*) "what we do is a function of what we think," and (*b*) "what we shall think is a function of what we have done"—formulated in Sect. 57—cover the case, provided we admit that the 'functional selection' of movements from movement variations—constantly repeated from a progressive 'platform'—is the actual method of motor accommodation. In the case cited by Mr. Bosanquet (*loc. cit.*, p. 174)—the building of his new house—I should say that the plan of the whole is made up of parts each of which is taken imitatively from other houses or plans of houses, or selected out by the owner himself from alternative variations of thought, by the process of getting new workable combinations, which is indicated above. I could not wish a fairer example.

How, I may add, Mr. Bosanquet can hesitate, as he says, in re-

gard to possibly classing my humble self as an 'associationist' I cannot imagine. All my psychological publications have been from the first as diametrically opposed to associationism as an apperception theory, based on motor unity and synergy, can be. I am also of the opinion that Mr. Bosanquet will find in the later writings of Dr. Stout, from whom he takes the theory of 'relative suggestion,' evidence that that able writer is inclined to supplement his view, on the genetic side, by a theory of motor selection.

V. The Science of Society.[1]

To the Editor of Science: Your kind question as to whether I have any remarks to make on Professor Giddings' article on my book in *Science*, January 6, leads me to send the few sentences which follow. I should not otherwise have done so.

I have no essential alterations to make in my book on the topics Professor Giddings brings up. I find in Professor Giddings' 'consciousness of kind,' even more now than when I criticised it in my book, and for the reasons there given, the 'climax of descriptive vagueness,' seeing that I am conscious, in view of his successive statements, of a certain tendency to agree with Professor Small, that Professor Giddings uses 'consciousness of kind' as a sort of prospector's claim to anything which may hereafter be discovered. Indeed, Professor Small's review (*Amer. Journ. of Sociology*, January, 1899) of Professor Gidding's recent *Elements of Sociology* anticipates my reaction upon much of the latter's writing. When a third party informs one that one's preserves are poached upon, one does not mind saying one is oneself aware of it![2]

The truth is that Professor Giddings' way of treating psychological questions, together with the sources of the earlier opinions upon which he is now attempting to engraft the results of later psychological research, are so remote from my methods and sources that I fail to find, despite the best will in the world, much common ground for debate. For example, Professor Giddings says in the *Science* article that I just

[1] Written for *Science* (under date January 10, 1899), but not published. I disliked the personal, and judged Professor Gidding's review to be after all ephemeral. But he has now incorporated it in his work, 'Democracy and Empire,' and I change my mind and publish this.

[2] On the whole Professor Small's article accurately expresses my opinion, even more now (1901) than when this was written, of Professor Gidding's work, and I do not hesitate to express this conviction, since in the present state of sociological study, sober, patient, and accurate research—with the temper of it!—is the great desideratum.

missed making a 'really important contribution to social science,' and then, seemingly goes on to make it himself, for with his 'Elements of Sociology' there was actually issued a circular calling especial attention to his 'important developments of sociological theory,' and to 'a new contribution to psychology no less than sociology'—*on this very topic!*—or, at least, on this topic so far as I am able to judge. For although I find what Professor Small none too bluntly calls 'poaching' upon the preserves of Ward, Patten, and myself,[1] I cannot make out what the discovery is. Instead, I find, especially in the chapter of the 'Elements,' vague Spencerian analogies, and a show of novelty and finality imparted by dogmatic statement and the use of new terms.

Finally, I suggest that all of us, who think to do work in the borderland between two sciences, study to be informed each *in the other's Fach*, no less than in his own. I say this so fully aware of its homecoming thrust, that if Professor Giddings' colleagues confirm him in pricking some of my *sociological* bubbles, I shall let them collapse without a murmer; but the psychology—*das ist eine ganz andere Sache!*

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THE IMAGE AND THE IDEA.

Recent discussions in psychology and philosophy have revealed a fundamental source of difficulty in both disciplines. From the philosophic side the difficulty has been brought home by Mr. F. H. Bradley's very thorough treatment of the relation of 'content' to 'existence.' For psychology the question may be formulated in terms of the relation of images to ideas. It is with this latter aspect that we shall at present deal.

Since Galton's day the image has been carefully investigated and for our present purpose it will be sufficient to summarize the results reached. It is now generally agreed that although there may be an identity of function in the ideas of men, they nevertheless vary, structurally, with the individual, his time of life, his particular task. This distinction between the individuality, the warmth and coloring of

[1] To cite a case—besides those pointed out by Professor Small—in this immediate connection, Appendix D in my book may be referred to as putting in my way certain things that Professor Giddings puts in his way in the *Science* article. Even certain of my terms (as Professor Caldwell also notices), such as 'socius,' 'organic' and 'reflective' sympathy, are used with no intimation of their origin. In what he calls 'ejective interpretation and selection' I find, if I understand his meaning, partial statements of elements of my 'dialectic of personal and social growth,' yet stated—as is the case also with the other conceptions cited—in a form which I as a psychologist should not wish to have attributed to me.

structural processes and the colorless and prosaic character of determinate intellectual functions has been permanently recognized by the use of the terms *imagery* and *ideation*. Imagery is a name for concrete mental processes taken in their immediate and varied individualities. Distinct images are emphasized portions of this process, what James designates substantive states. Series of images constitute the imagery of separate moments as emphasized factors arise and pass away. These images vary in character and coloring to an indefinite degree, although typical forms predominate in each of us.

Ideas are not so easily described. To the elucidation of the nature of the idea and of its relation to the image the remainder of this essay must, therefore, be devoted.

Although the logical and metaphysical values of ideas have been exploited over and over again their psychology still requires much to be done. Even when we identify the idea with the concept we are safe in saying that the psychology of the process is still fragmentary.

As a point of departure the commonly received doctrine that *ideas* are *signs* or *symbols* may be accepted. As such they embody the *meaning* developed in the reflective activities of mind. But what are signs and how do they symbolize? Here the logicians come to our assistance. They have made clear to us that the meaning which universally constitutes signs consists in reference and in the relationships thus dynamically established. Every concept or idea is constituted by the sum-total of relationships embodied in its meaning. Granting this, the question arises, "How do relationships and references *exist?*" And at this point interest becomes acute, for over this question sensationalists and intellectualists have battled for ages. For the former, the reality of the idea has reduced itself to the rapid succession of a series of separate and more or less imperfect images. Relationship, meaning has either been done away with or reduced to a subordinate function of images. Every one is familiar with the challenge of Berkeley and Hume to show how else than in an image or a series of images the concept can exist. "Lay the finger," say they, "upon the concept and it melts away into images." The intellectualist, on the other hand, fearful of the consequences of sensationalism and persuaded of the inadequacy of separate images to explain the unity involved in meaning and relationship, has usually felt constrained to speak in a most mysterious way of the manner of the idea's existence. Instead of answering directly the psychological question of the sensationalist he has changed ground constantly and has expended his energies in proclaiming the necessity and value of related meaning. For this reason

the problem claims renewed attention. Contemporary psychology and logic have advanced the inquiry considerably. Logicians have pointed out that relations without terms, that reference without a base, are absurdities. Thus the intellectualist is forced to give a concrete answer to the question asked of him and to speak to the point. In this the psychologist is quite at one with the logician. On the other hand, the logician has conserved the true nature of meaning, reference, relationship in the presence of fleeting images. It has been demonstrated that before images can take on the function of reference, before they can become vehicles of relationship, they must be tranformed in a synthetic function which (metaphorically speaking) lifts them out of their immediate and individual natures. As we now view matters, the difficulty with both parties has consisted, first, in working from different points of view without knowing this, and second, in each setting up his own point of view as the only possible. The sensationalist has worked from an analytic standpoint and the intellectualist from a synthetic. For the former the organized whole—the idea—when dissected disappeared into separate terms or elements; for the latter no elements could constitute an organized whole or function apart from a synthetic act which gave vital unity to them. Thus we may admit that when we attempt to lay our fingers upon concepts they resolve themselves into flights of images, while at the same time we insist that there is a difference between an unmeaning succession of images and the idea of succession wherein meaning is realized, and that this latter involves a synthetic act by which the separable factors are organized in a related whole.

At this point the deliverances of psychology and logic become somewhat uncertain and we find it necessary to break new ground.

So far we may be agreed that ideas as symbols present a twofold aspect. On analysis they reveal the presence of images which constitute a base; taken as wholes, a synthetic function presents itself, a function whose dynamic nature is constituted by references or relationships. We must now make a more careful survey of these aspects.

And first it is to be noted that the relation between the base and the reference, the image and the relation, is inner and vital, not outer and mechanical. The two are separable factors and not separate elements. In other words, the psychical reality is a single activity, the symbol which for purposes of convenience may be viewed in two different ways. Image and reference being thus two different aspects of the same existence, we have answered one part of the sensationalist's question by agreeing with him in his contention. Ideas *exist* in

images. But when we press the question and ask how they exist, introspection makes apparent that images furnish simply the *base* of the idea, and the complete symbol involves primarily a synthetic function of reference into which the image is taken up and absorbed. Hence, in all processes of ideation imagery is constantly overlooked, and whenever meaning absorbs our attention the concrete characteristics retreat so far into the background as to fade altogether from view. We may illustrate the matter in this way: When we use a sign we never stop to consider the stuff of which it is made, but pass on to the objects which it indicates. Its meaning is, for the time being, our concern. But if for some reason we desire to examine the structure and make of the sign, we become aware, at once, of the stuff and its character. Stuff and reference, base and meaning, are (in fact) inseparable, although in normal use the stuff is subordinated absolutely to the reference. We know of no signs, no matter what their order, which exist apart from some sort of base. And yet mere stuff has no significance for us.

Having discovered that ideas are always based in images, we must next examine into the precise nature of the function which the image supports.

As already noted, the idea-function is constituted entirely of references, relationships, meaning—what James designates 'transitive' states. Now meaning or significance lies in determinate pointing—and this in two ways. First, the reference may involve merely the relation of one concept to another. Sign points to sign; meaning implicates further meaning. Second, the reference may involve not another abstract, but something concrete and immediate.

The first form of reference has to do with the interconnection of generalized meaning. The more determinate ideas become, the more general is their signification. Instead of being tied down to the limitations of particular times and places, the meaning is freed, abstracted. The more determinate scientific work becomes, the more completely it embodies itself in general, abstract formulæ. Furthermore, the human mind endeavors universally to organize these abstract meanings. Every idea as idea involves relationships beyond itself. And it is the part of systematic thinking to enlarge these relationships until each idea has been woven into a larger whole, constituted by the complex interrelationships of the component ideas. The ideal of abstract thought is to discover completely the relationships of each and every idea, *i. e.*, its real meaning. Granted then that the significance of ideas consists partly in the inter-reference of symbols, the

question arises once more, 'How is abstract meaning constituted in mind?' Again we are against the problem of the sensationalist and the intellectualist. And once again we must answer as before. It is impossible to believe, with the intellectualist, in what might be called a purely disembodied idea, one devoid of all connection with imagery. The most careful survey of mental states fails to reveal any such. The moment we analyze meaning, we find ourselves in the presence of imagery—visual, auditory, tactile, motor. On the other hand, imagery as such is not ideation. As reference is embodied in the connection of states, one with another, so meaning lies in the forecast and control of a series of states. It is quite true that every *meaning* involves the use of images, but the function of projection, its form and actual course, is a dynamic activity, which, if it cannot be separated from imagery, neither can it be resolved into mere imagery. If we are exact we shall realize that the meaning is the complete projected experience, consisting, from one point of view, of a series of imagery, from another of a dynamic transition from one image to another. In support of this general position several lines of argument may be advanced.

1. From a logical point of view the significance of ideas is divided into denotation and connotation. By denotation is understood the range of an idea's signification; by connotation the inner character of the signification. Psychologically the distinction is indicated by the use of the terms *general notion* and *abstract idea*. So far as the former is concerned, examination makes it apparent that the general notion is made up of a series of fleeting and more or less imperfect images. The general notion of triangle is represented (for the visualist) by a series of images of variously formed triangles or parts of triangles. Of these, the angles may be most pronounced, enlarging and expanding as the meaning may demand, while the sides may be faint and rapidly changing lines of connection. When we inquire into the connotation of 'triangle' a similar result is reached. A triangle is roughly described as a three-sided figure. Now this definition may be entirely verbal to us, or it may be real. In the latter case the presence and significance of imagery become apparent. In the case of the visualist, the appreciation of meaning involves the projection of three lines in such form as to enclose a space: and, so far as the connection of the abstract idea with the general notion is concerned, it is really *seen* that the variation of form does not detract from the use of the lines.

I have referred throughout to the representation of the visualist.

But although we must always reckon the differences of imagery in making up our account, such reckoning but serves to emphasize the general point which we have been making.

2. The second line of argument is founded in what at first hand appears to be a discrepancy. It would appear at times as though thought were constantly being carried on without reference to imagery. The more abstract our thought, the more habitual our lines of investigation, the more rapid the consideration of the moment, the farther removed imagery would appear to be. The source of the difficulty is not far to seek, however. It consists in the substitution of conventionalized for the more natural forms of imagery. The pre-eminent avenues of expression are the voice and the hands. It is no great wonder, therefore, that in our thinking, the imagery connected with these should in the average individual become predominant in thought. When, therefore, in abstract thought or in rapid reading ordinary sense images fade away, it is not that imagery has been abandoned altogether but that a new form has been substituted. Furthermore, the fact that in rapid reading or thinking imagery does not obtrude itself upon attention, is not to be wondered at. As has been remarked above, the *structure* of a symbol is always submerged in the function which it serves. Consequently, the more we think, the less we notice imagery. That in abstract thought imagery should be out of sight is precisely what we should expect.

3. Another proof of the close dependence of meaning upon imagery, is found in the constant resort to imagery when thought is baffled. So long as we use symbols which are quite familiar and so long as the combinations made from them fall within the beaten tracks of experience and habit, we pay little attention to the flights of images which bring home the significance of our thought. A mere suggestion is sufficient; our thought takes the required turn and we pass on. The moment, however, that some new thought or some new combination of thoughts arises, we search for the concrete imagery in which the conception may be appropriately embodied. Until we set the imagery before us, the thought remains vague, we are baffled. When the precise imagery flashes into mind a distinct sense of relief comes over us. The more vivid the picture, the greater is our assurance. Finally, if at any time we wish to fix a thought or to correct a mistake we turn almost involuntarily to the imagery and emphasize that. It is imagery which ensures a final *realizing* sense to ideation.

4. The study of aphasia furnishes additional grounds for a belief

in the inseparability of imagery and ideation. Destruction of centers upon which imagery depends is just as fatal to the existence of meaning as the destruction of the connections between centers upon which relational activities depend.

In the most abstract forms of thought, therefore, image and idea are inseparably connected. Meaning exists as a dynamic function of projection by which further experiences and thoughts are indicated. This projection, however, involves the use of imagery as a base, and, although the imagery is taken up and submerged in the function which it embodies, it is nevertheless present at every moment of thought. As we consider the next point this position will become more explicit.

In treating of the reference-function which constitutes the meaning of ideas it was remarked above that the reference might be of two forms: first, the reference of one symbol to another; second, the reference of the abstract symbol to something entirely concrete and immediate. Having examined the first form we shall now turn to the second.

Although reflection leads more and more to the determination and organization of abstract and universal symbols it still maintains its connection with the concrete and immediate aspects of experience. It is now commonly agreed that universal meaning cannot be separated ultimately from particular significations. Logicians emphasize the fact that universals do no more than set forth the identity of meaning observed in a variety of particulars. Scientists are fond of impressing upon us that laws have no meaning except as statements of the general methods of behavior discovered in facts. Psychologists also realize that no hard and fast line can be drawn between perception and conception. The distinction made between particular and universal, fact and law, percept and concept, is indeed not one of meaning, but one based upon the *use* to which the same meaning is put. Whenever meaning is limited to a single portion of space or time we have a concrete, a particular. When we abtract meaning from such limitation and use it in a free way to light up an indefinite number of singulars we have a universal. In other words every particular is a 'that-what' in which the 'that' stands for the discriminated images while the 'what' embodies the meaning, which, although it enlighten this image, may also enlighten an indefinite number of others. It is thus correct to say that ideas arise directly from immediate undiscriminated experience. The problem which confronts is, "What part does the idea ultimately play?" This will enable us to understand more clearly how the idea *exists*. In answering the question it is essential

to note immediately that the *significance* which departs from, refers beyond, transforms the immediate image, finds its function in conducting us back to concrete experience. Knowledge does not consist in framing copies of extra-mental existences, but in developing the function by means of which we are carried from one form of experience to another. The 'objectivity' of the function consists in the fact that although in one sense the knowledge function and its termini are absolutely individual and 'subjective,' they are at the same time and equally universal. In other words, there can be a subjective synthesis for us only because we lay bare a deeper, yet immanent order which constitutes the inner reality of the movement which is our experience. For the principle of this distinction we must thank Kant and his followers of the positive school. Its working out has not yet been fully accomplished. In it, for example, we find the strong and weak points of Mr. F. H. Bradley's system. Underlying this thinker's philosophy is a definite view of knowledge and of its relation to immediate experience. According to Mr. Bradley every idea is a transformation of immediate experience. Knowledge thus rises above and departs from the actuality of given reality. The more determinate ideas become, the more difficult therefore is it to understand how knowledge can be other than a mutilation of the real. Mr. Bradley recognizes fully that the emphasized image or 'that' is stripped of its individuality when it enters into the idea-function. 'Content' may embody the most determinate forms of meaning, but nevertheless it has deprived 'existence' of its own nature and has imposed another and arbitrarily, as it must appear, unless we can carry the investigation one step further. This difficulty even the average Neo-Kantian has failed to recognize. He still trades with the thought that universal and particular, identity amid differences, are necessarily correlative distinctions within knowledge. The root of the matter, viz., that the meaning which is identical in both particular and universal, differing only in its use or appearance, is a transformation of immediacy, which must appear as arbitrary unless the complete function of knowledge can be laid bare; this root, I say, escapes him. Not until we realize that the idea function which separates us from the immediacy of experience aims in the same movement at connecting us with immediacy can the difficulty be removed and knowledge be given a legitimate work to accomplish. To illustrate this point let us analyze the concrete judgment: "That is a crayon." In our vison of the crayon the 'that' is embodied in the factors of brightness and color fixated by our gaze. As a factor in the complete judgment the 'that' therefore

is constituted by the image plus the initiation of the transformation which constitutes the 'what' or meaning. The 'what' in its turn embodies the references of the image or more specifically the anticipations of other possible concrete experiences, other 'thats'—sensations of pressure, of sight, etc., and also of the activities through which these possible experiences may be actualized. The idea is therefore a transitive state which in its movement controls the experience of definite substantive states.

Ideation as reference is embodied in determinate forms of activity which in their initiation and discharge implicate the concrete imagery which furnishes the base for each discharge and illumines its course. The projection of images which indicate the immediate experiences to be realized constitutes our foresight or self consciousness; the lines of discharge constitute the conditions under which the concrete experiences may be realized. Looked at as a psychical event, the idea, therefore, is always a symbol which operates upon a basis of imagery. As a symbol the idea embodies the self-conscious references or relationships which constitute on the one hand our exact anticipations of possible experiences and on the other the necessary conditions under which our anticipations can be realized. For this reason ideation not only is but must be self-conscious and foreseeing. But again, our foresight or anticipation is embodied in imagery whereas the governing conditions are constituted by determinate habits of discharge. The conclusion of our earlier investigation is thus sustained. Imagery and ideation cannot be separated from each other. The thought function includes both. In the initiation of the mental discharge imagery is entirely subordinate; in the process of discharge it comes more and more to the fore, until finally its clear and determinate character constitutes the anticipation which makes knowledge a matter of intelligent direction in life. If, further, we turn attention to the part played by ideation we realize at once the true character of the criterion of knowledge. This does not rest in the vividness or intensity of the involved imagery, as some have supposed, nor again in the consistency of ideas one with another. These have their value, but that value is not fundamental. Intensity, vividness, clearness of imagery give a 'realizing' sense to our pale conceptions but this may be a source of illusion, *e. g.*, in our dreams. Consistency is of extreme value in the manipulation of ideas already formed and in the framing of new combinations from old material. It does not assist us, however, in the original determination of ideas. The natural test of knowledge rests in an inquiry into what the idea attempts to per-

form. As a symbol, it professes to set before us the conditions upon the fulfillment of which certain determinate experiences may be realized. The criterion of the truth of the idea, therefore, is: "Does it realize its pretensions?" Of this daily experience and the work of science are proofs. And at this point we may recur briefly to the treatment of 'objectivity' in knowledge. According to the remarks made immediately above, an idea is objective when it realizes its anticipations through their appropriate conditions. This is true universally. There are many kinds of knowledge and many spheres of existence, but in every case reality is distinguished from illusion on the basis of the reliability of our intellectual symbols. In connecting this with previous statements concerning 'objectivity' it will be sufficient to point out that in the development of symbols it has been discovered that mere subjective wish or whim must be subordinated to the discovery of a method of framing anticipations and of determining conditions which are given to us through sense experience and are not arbitrarily imposed by us. To the discovery of this determinate order latent in every aspect of existence science universally directs itself. And, as Kant pointed out, in the recognition of this immanent order as the reality of every existence is found the true answer to Hume's question concerning the necessary and universal significance of ideas. Experiment thus remains as the natural and ultimate test of knowledge. Having formulated our anticipations and wrought out the corresponding conditions to the best of our information and experience, the last step remains to find whether the conditions when set in operation realize concretely the expectations which we have framed. If such experiences are realized for us as we expected, and if they are realized after the manner anticipated, our ideas are true and real, knowledge is attained. If our expectations fail, then in the sense in which alone knowledge has meaning for us, the idea is false. With this result science agrees, as through the experimental method and that alone it has built up its great fabric.

We have now examined the different aspects of the structure and function of ideas. Meaning, whether embodied in abstract universals or in concrete particulars, has turned out in each case to be symbolic. Symbols in their turn have turned out to be principles of anticipation and control of concrete experiences. Knowledge has a distinct and objective function, but that function never consists in furnishing us with copies good or bad of an extra-mental reality. Its function is rather that of bringing to more determinate consciousness the reality which is already in mind although in an indeterminate form. Put

otherwise the function of knowledge consists in developing reliable symbols of control in the anticipation and manipulation of possible experiences. Thus, on the one hand knowledge is relative and on the other it is objective. Judgments are alike hypothetical and categorical. Every fruitful idea contains essentially a condition and also a determinate expectation. As relative, knowledge grows and differentiates with the development of reflective critical experience. It does not profess to set forth the complete and possible content of reality but to determine the regulative principles of the varying aspect of reality. As objective, it places the criterion in the control of anticipation by actual conditions.

We can perhaps close this essay best by squaring accounts with association and transcendental interpretations of ideation.

The associationist asserts that the process of ideation can be resolved into a series of images related together in determinate forms. And from one point of view this interpretation is entirely correct. If we are looking toward the reproduction of meaning *already established* we must analyze it into substantive states, dynamically connected. If, however, we ask ourselves how meaning was originally established and what it does functionally at any moment, we must maintain unreservedly with the transcendentalist that association does not account for the origin of meaning and that it is no more than a record of meaning previously developed. Meaning involves a consciousness of the unity present in all relationship. Fundamental therefore to all significance is that synthetic activity which makes the function of reference possible. The mistake of both schools has consisted in tearing into two the single process of ideation and in setting up the mutilated parts as though they were each the living whole. Ideation involves the presence of both analysis and synthesis: the development of symbols universally takes into account the reproductive results of past gains. Imagery and reference, terms and relations, cannot be divorced. And this is the final word of insistence—only when we realize the presence of imagery as the embodiment of symbols, and of reference as the spirit of control for imagery, can we understand the real nature of ideas and keep clear of the rocks on either hand which have wrought such woe to thought.

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PSYCHOLOGICAL LITERATURE.

Les Dilemmes de la Métaphysique Pure. CHARLES RENOUVIER. Paris, Félix Alcan. 1901. Pp. 283.

The evil plight of metaphysics, where every assertion is met by a counter-assertion and where disputes seem never to be decided because there are no principles accepted in common by the disputants, has long been a favorite theme with the sceptic, triumphing in the proof that everything in this region is sophistry and illusion, and with the philosophical reformer, urging his new method by which metaphysics was to be brought into the sure path of science. But in spite of the confident denials of the one, men still continue to speculate, impelled presumably by the ineradicable metaphysical need inherent in human nature; and in spite of the explanations and hopeful predictions of the other, the contradiction in principles was never perhaps more acutely felt than now. Not the least important undertaking in philosophy is the attempt to reduce the many contradictions to a certain few fundamental contradictions and to discover, if possible, the ultimate logical presuppositions from which they spring. This is part of what is attempted in the work before us; the other part is to suggest the way of escape.

The fundamental contradictions that underlie all systems of pure metaphysics are, in Renouvier's opinion, the following five: (1) The assertion of the unconditioned—there exists being *in se*, unconditioned, necessary and unknowable, to which is opposed the assertion of the conditioned—there is no being *in se*, but all that is is relative to phenomena and to the laws of our cognition; (2) the assertion of substance—there exists *in se* an indefinable and unknowable entity, the seat of qualities, to which is opposed the assertion of law, or function of phenomena—substance is nothing but the logical subject of qualities and definable relations; (3) the assertion of the infinite—there are concrete quantities, *e. g.*, the series of events in time and of objects in space, the really distinct parts of which do not make up numerically determinate wholes, to which is opposed the assertion of the finite—what is really composite, and hence the actual series of past events, the world of objects in space and the whole world of beings generally, consists of a finite number of really distinct parts; (4) the assertion

of determinism—every phenomenon is completely determined by the given fact of its antecedents and circumstances, to which is opposed the counter-assertion of freedom, which denies this; (5) the assertion of things—consciousness and persons are products of the world, to which is opposed the assertion of persons—consciousness is the principle of both knowledge and being, and the world is the product of persons.

Now these antitheses constitute, as they are here represented, genuine dilemmas, for the opposed propositions, it is contended, are in each case mutually contradictory; neither can be refuted by principles admitted in the same sense by the partisans of both, while yet, being true disjunctions, choice between them is inevitable. It is further contended that the successive propositions mentioned first in order, when taken together, form, with their corollaries and consequences, one logically coherent system, while those opposed, with their corollaries and consequences, form another. And inasmuch as the latter, the theses of the conditioned, law, the finite, freedom and persons, make up the framework of Renouvier's own system, there is the implication that ultimately we are forced to choose between that and every other. The logical principles from the acceptance or rejection of which, according to Renouvier, these contradictions spring, are the principle of relativity, which asserts that no knowledge is possible or existence conceivable except through relations, and the principle of contradiction, or the rule of the agreement or disagreement of relations respecting the same subject. The first principle is rejected, and the second also, so far as it implies the first, by the advocates of the propositions on the one side, the realists and necessitarians, while it is the very foundation principle of their opponents, the phenomenists and libertarians. The solution of ultimate questions on pure grounds of logic is thus declared to be impossible. So much Renouvier concedes to the sceptic. What then does he propose as the philosophical reformer? Simply, the free choice of fundamental principles. Freedom is thus presented, not merely as a practical belief, the foundations of which may be invalidated by science, but as a rational belief, which serves as a principle of theory and of all knowledge.

Renouvier has written nothing better or more characteristic than this little book. It is in its way a philosophical masterpiece, one well suited to introduce our students to a school of thought that has long been influential in France, but which has hitherto received little attention here, in spite of the public acknowledgement of his own in

debtedness to it made by our most distinguished American psychologist. And Renouvier, it may be added, is worth the study, not least perhaps because he so often compels us to disagree with him, a philosopher subtle as Lotze, stimulating as Schopenhauer, clear as Descartes. Effective criticism of a work like this involves discussions that would here be out of place. The psychologist will probably find a point of interest in the author's rejection of a soul-substance on metaphysical grounds, especially in its connection with the assertion of personal freedom, a connection which, if admitted, would seem to involve important consequences alike for the psychologist and the man of science generally, consequences subtly suggested by the dedication of the book to Boutroux, the author of the 'Contingency of the Laws of Nature.'

H. N. GARDINER.

SMITH COLLEGE.

La Mémoire. J. J. VAN BIERVLIET. (Bibliothèque internationale de psychologie expérimentale, normale et pathologique.) Paris, Octave Doin. 1902. Pp. 346.

The purpose of this book is, in the words of its author, 'to sketch the history of the problem of memory and its evolution during the last fifteen years.' He declares that he 'will glide over the various recent theories, however ingenious they may be, in order to dwell upon clinical observations, and to treat at length the research work undertaken in the various laboratories of experimental psychology.' By way of further limitation, he adds that he 'does not wish to make a complete critical catalogue of all the recent work, but to give a sufficient knowledge of the actual movement to prepare the reader to undertake or simply to comprehend original researches on memory.' Herein one may detect a conflict of aims that has led to a book missing somewhat either mark. On the one hand the reader can scarcely feel assured that he is made sufficiently well acquainted with the work done in any special field to undertake forthwith an investigation calculated to advance our knowledge thereof, while he who hopes merely to be able to comprehend works of original research may find so many elaborate accounts of experimentation a trifle tedious. Furthermore, a novice in these regions will feel aggrieved that the limit of fifteen years has shut out anything save the barest mention of the work of Ebbinghaus and any notice whatever of that of Wolfe.

Professor Van Biervliet's book, however, is well timed and will serve the useful purpose of furnishing a transparent and fairly well

selected account of recent experimentation on memory and observations upon its derangements. He uses as the basis for the divisions of his treatment the well-worn analysis of memory into retention, recall, and recognition, the last including localization. However excellent this may be as a classification of the factors in memory, one can hardly fail to be at first glance a little skeptical about the success of its application to a collection of studies most of which are complex enough to involve some sort of suggested conclusions about nearly everything connected with memory at all. I am bound to confess, however, that in M. Van Biervliet's hands it has succeeded far better than I had anticipated, and by it he reduces a heterogeneous mass of material to at least the appearance of order. To suppose that we have more than this may, however, lead to questionable conceptions. When, for example, experiments on the power of children to reproduce immediately a series of numbers or words or the like are classified as dealing with the intensity of the memory of fixation, one is doubtful whether the results may not be due to such elements as habit of attention, familarity with material, etc., quite as much as to mere retentive power. In this event the experiments belong under the division dealing with association with as much justice as experiments on the formation and breaking of habits, which are thus classified by M. Van Biervliet. Indeed, except where there is undoubted destruction of cortical tracts with complete loss of specific images dependent thereon, we are offered by the author no clue whereby we can determine in how far the forgetting of a thing is due to failure to retain rather than to a failure to associate. The experimentation cited tests generally, I am prone to think, the permanence of associations instead of any retentiveness separable therefrom.

M. Van Biervliet devotes nearly one-half his book to 'the memory of fixation,' dividing this part into three chapters, which deal respectively with the seat of memory, its types, and the intensity of the memory of fixation. In the first of these, while discussing retention as a general function of matter and particularly of the human body, he affirms roundly the existence in the germ of memory for the habits acquired by the parent, and ignores silently the doubts and arguments of the Neo-Darwinians. He then sketches rapidly the development of the theories on the localization of functions of the brain, and passes to a somewhat more detailed discussion of the history and present state of our knowledge of the nervous apparatus involved in using language. He notes that motor aphasia can be separated from agraphia, and amusia from aphasia. The second chapter, on the types of memory,

leaving the work of Galton unmentioned except for a minor contribution, begins with the classification of Charcot, and suggests that types may be based not only on the sense involved, but also on social rank, race or sex. The bulk of the chapter is occupied by reviews of the experiments of Cohn on the coöperation of acoustic-motor and visual memories, Toulouse's examination of the imagery and methods of remembering used by the novelist Zola, and Binet's book on great calculators and chess players. The importance of the motor element is emphasized, and its relation to the predominantly visual type on the one hand and the auditory type on the other is well brought out. The author remarks the complexity of the elements involved in most individual memories, and shows how certain of these are often strengthened because of their use for professional purposes. The third chapter reviews experiments on the effects on power of recall of race, age (children being tested), repetition, distraction, rhythm, etc., with studies on the memory for shadows and distances and of muscular memory for length of lines. Smith's experiments on muscular memory are here chronicled, though differing little from those of Cohn, which are dealt with in the preceding chapter.

Part Second, on recall, has two chapters, the first of which takes up the subject of maladies of the reproductive memory. Here we are concerned chiefly with the disappearance and reappearance of groups of memories and the phenomena of multiple personality. A second class of cases, where there is progressive destruction of all associations, is also discussed. Experimental researches on association are taken up in the second chapter. Most stress is laid on the work of Bourdon and Aschaffenberg, though results reached by Bergström, Jastrow, Miss Calkins, Ziehen and others are mentioned.

Part Third discusses recognition, and localization of memories in time. It follows the general plan of dealing first with pathological cases and then with experimental work. Some most interesting material on the false recognition of things experienced for the first time is introduced. The cases range from that occasional feeling so common among us of having mysteriously experienced *just this* before, to an instance where the whole psychic life seemed to be a duplication of the experience of a preceding year or even a reduplication of that duplicate experience. The chapter on experimental work deals with five contributions, but devotes most space to Bourdon's work on the recognition of repeated letters in a series and Vaschide on the localization by memory of the words in a series.

In conclusion, along with various precautionary suggestions as to

method, the author expresses particular distrust of results reached with trained psychologists as subjects. As a parting word, he holds out the hope of a rich reward to the experimenter on memory in the practical application of his results to education, and notes the so-called 'word method' of teaching reading as an illustration of this.

In spite of an endeavor to represent impartially the work done in the various laboratories of the world, the contributions of the French are emphasized somewhat unduly. More than half the writings that are given anything more than mention are in the French language, while nearly half the authorities cited are French. Moreover, while we might expect them to receive the lion's share of the part devoted to discussion of pathological cases, we should hardly think their laboratory work would require decidedly more space than that given to others. It is to be regretted that such studies as those of Münsterberg on habits, Bryan on motor ability, Netschajeff on the memory of children, and Lay on mental imagery should have been unnoticed; also that there should have been no mention of what has been done on memory for tones and on the earliest memories of childhood. Moreover, while there has been but little experimentation on the quantitative and qualitative effects of considerable intervals of time on our memories, the importance of this field makes it deserving of a special mention such as M. Van Biervliet does not give.

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BINAURAL HEARING.

Ueber binaurales Hören. GINO MELATI. Philosophische Studien, XVII., 431.

The above contribution from the Leipzig Institut commends itself at once to the reader as a report of well planned and carefully executed experiments. The chief object of the investigation was to submit to a fresh and particularly rigid examination the phenomena of binaurally perceived beats. May these occur when every precaution is taken to exclude the transmission of excitations from one ear to the other either through the air or through the bones of the skull? And if beats may be heard under these conditions, how do they differ from those heard monaurally? In the process of answering these questions several collateral matters were incidentally investigated also.

The desired conditions were secured first by the use of three separate rooms, the subject being placed in the middle room and a tuning-fork in each of the others. The tones from the forks were conducted

to the middle room through brass tubes, in the ends of which were inserted pieces of glass tubing which could be slid in and out and thus be inserted in the subject's ears. The forks were of 500 vibrations and were electrically actuated. The tone was thus continuous, and by means of the adjustments used could be kept constantly at any grade of intensity desired. The pitch of either fork could be varied by very acurately adjusted running weights. Every precaution was taken, by means of paddings and wrappings, to exclude extraneous sounds, and the reader readily shares the author's conviction that only the pure tones of the forks reached the subjects' ears.

But now the conditions which excluded the transmission of sound from one ear to the other. Transmission through the air was easily excluded by using tones so weak that no sound could be heard by either ear when the glass tubes were not inserted in the ear passages. Transmission through at least the outer bones of the skull was held to be excluded, partly because tones of this low intensity could hardly possess energy enough to carry vibrations over the entire surface of the skull, but particularly because of the introspective testimony of the observers on two points. First, if both tones be sounding and one ear be stopped by the insertion of the finger, no beats must be heard, under correct conditions. If heard, the intensity of the tones must be still further reduced until the beats vanish. Secondly, one ear being closed by the finger, and the tone upon that side being then interrupted, the subject is asked to note any change in the tone heard. If the reply is given that the tone has become less full in character, this is taken as evidence that the influence of each sound is confined to the ear upon its side.

The desired conditions being satisfactorily secured, and their maintenance carefully and frequently controlled, the four trained subjects were called upon to note every possible phenomenon and to make constant comparisons between the binaural and the monaural experiences. The experiments were begun with mistunings of the forks which produced from 2–15 beats per second. In later experiments the difference between the vibration rates was much further increased. The particular results in connection with the perception of beats may be briefly stated:

Binaural beats are less definite in character than the monaural and are less clear. They are more subject to fluctuations of the attention.

Binaural beats are of less intensity than the monaural, the maximum intensity occurring when the beats are less than 10 per sec. The law holds that as they increase in frequency from this point they

decrease in intensity. Consequently, the point of greatest roughness is not, as in monaural hearing, at 30 beats per second, but rather in the region of 2–10 beats.

The extreme limit of binaurally perceptible beats is, in this region of the scale, at 50 per second, against 60 or 70 for monaural hearing.

The feeling of roughness is never so great in binaurally as in monaurally perceived beats, and it disappears entirely when the interval between the tones exceeds 30 vibrations.

Several further points, for the observation of which the experiments afforded peculiarly good opportunities, were carefully noted:

1. All the intervals employed were felt to be dissonant, though, contrary to Stumpf, in less degree than in monaural hearing. But the fact to be noted is that the dissonance remains beyond the point—30 beats per second—where the roughness has disappeared, and as clearly also in those cases where, with the tones just at the threshold of perception, all roughness and all beats too are wanting.

2. A subjective tone will beat with an objective tone. Stumpf denies this, but the author has the opportunity to observe this upon himself under carefully controlled and varied conditions.

3. Again, in direct opposition to Stumpf, the author finds that the grade of fusion of the binaurally heard tones diminishes as the interval between them increases. The degree of fusion is never so great as in normal hearing, and there is simply no fusion at all when the tones are at the threshold of perception.

4. The claim made by LeRoux, Urbantschitsch, Bloch and others, that binaural tones exercise upon each other a mutual reinforcement, was submitted to a thorough examination. The result found was that when the interval between the two tones is small, the intensity of each is certainly increased by the presence of the other. This is not the case, however, when the vibration rates of the tones differ by more than 10 per second. Then, the author states, one may rather speak of a mutual weakening of the tones. Bearing in mind the low intensity of the sounds employed, one must suppose that this mutual reinforcement of tones is centrally conditioned.

5. A few observations were made on the localization of the various impressions. Each of the binaural sounds was found by careful scrutiny to lie, not exactly in the ear on its appropriate side, but slightly within the head. Under these circumstances the beats were localized in the deeper intracranial region between the binaural impressions. When, however, the intervals were greater than 15 vibrations per second, the beats seemed to be located at some far distant point.

Schäfer's well-known observation to the effect that the closing of one ear will change the localization of a tone just perceivable by the use of a resonator, so that the tone wanders from the stimulated ear to the middle of the head, is subjected to an interesting criticism. The author suggests that the apparent shifting of the sound may be illusory, due to the disturbing influence brought about by stopping one ear. Such an illusion disappears with practice, and the author is convinced that a weak tone is invariably located within the head, whether the unstimulated ear be closed or open. To show the great reliability of his results, he appeals to the fact that the arrangement of his experimental conditions allowed a greater convenience and certainty of observation than in Schäfer's case. It is highly desirable that further experiments be made in this extremely interesting field of intracranial localization.

As remarked before, the whole article, the main points of which are summarized above, gives the impression of stating results and observations that are more than ordinarily reliable and worthy of attention.

A good historical survey precedes the report of the experiments.

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THE VISUAL PERCEPTION OF SIZE.

Die Form des Himmelsgewölbes und das Grösser-Erscheinen der Gestirne am Horizont. von Zehender. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Vol. 24. Pp. 218–284.

This elaborate paper contains valuable historical matter and a reopening of the whole question as to the vaulted appearance of the sky, and the apparent enlargement of the sun and moon near the horizon.

The historical parts of the paper, however, make no claim to completeness, and in fact Lipps's theory that binocular vision is an important element in determining the appearance of the heavens is not referred to. The author goes back to Malebranche, and also devotes a special appendix of considerable length to a curious *Promotionschrift* by Treiber, in which, as early as 1668, the vault-appearance of the heavens is attributed to mist and clouds forming an actual concave overhead, concentric with the earth's surface; and by computation the altitude of this vault is set down by him as one-tenth the radius of its base.

The author's own view on this first question is, in the main, the same as Treiber's. Clouds and mist are the prime source of the vault appearance; without these there would be no direct impression of vaulting whatever. The clear blue sky, he holds, is of itself formless, although indirectly, as a whole, it gives a vague sense of rotundity, in part carried over perhaps from observing the clouded sky and also due to our natural tendency to represent infinite space as a sphere. And, again, the starry sky suggests to the author a formless swarm of points, and not a vault or surface. The low vault of the clouded sky is consequently no illusion, but is the truthful appearance from below of the concave of vapor concentric with the earth; knowing the height of the stratum we can compute the actual form of the sky. Evidence that mist and vapor alter the apparent form of the heavens is gained from careful quantitative experiments by von Sicherer on the bisection of the apparent arc from zenith to horizon. These results show that in dividing the arc by eye, one puts the mid-point nearer the horizon in cloudy weather (37.7° high, on the average, as against 42° for cloudless days) indicating that the arc seems depressed when the sky is overcast.

On the second problem—the illusory change in the size of the sun and moon—the author is sure that this is not to be explained by the occasional low-vault appearance of the sky. He does not seem so certain of the positive explanation. Several factors are dwelt on; and first, the greater atmospheric clearness toward the zenith, which makes the object seem nearer and consequently larger (although the author also cites without reconciliation Zoth's questionnaire results, showing that for most persons the sun and moon actually seem farther off at the zenith). He also feels that the effect toward the horizon may have some connection with the well-known over-estimation of an extent that is minutely subdivided. That the direction of the line of sight is also an important factor is shown by his summary of Zoth's and Stroobant's experiments on the comparison of lateral extents on a level with the eyes and overhead. The evidence is clear that extents overhead seem smaller.

But perhaps the main emphasis is laid on what the author believes to be a predisposition to regard universal space as spherical, to which I have already alluded. This tendency, it seems, leads us also to view lines that are perpendicular at the horizon as great circles of a sphere and as converging upward to the zenith. The sun and moon became involved in this system of imaginary meridians, and out of it all, in a way that is not made entirely clear, comes the illusion of diminished

size in the high heavens and of enlargement toward the horizon. The author feels that there is probably some connection between these facts and the observation of Volkmann that perpendiculars, if they are to seem parallel, must be made slightly convergent above.

The author's discussion of the shape of the heavens has in it much acute and convincing observation. After reading his account it is easy to satisfy oneself that the heavens are of decidedly different form according to lighting and atmospheric conditions; clouds especially make the vault seem low. And the cloudless sky by day, apart from the region of almost inevitable mist toward the horizon, has certainly a much less definite and perceptible form than the cloudy sky. But the present writer cannot confirm the author's statement that the clear night-sky shows only a formless swarm of stars. I get a decided impression of vaulting from the starry sky; and a number of persons, questioned in the evening with the facts before them, are unanimous that they similarly have a definite vault-impression. For myself, on especially clear nights, the impression has been of a *high* vault, the stars overhead appearing farther off than those below. Others, and I myself at other times, see the evening sky more as a hemisphere.

As to the low-vault appearance of the clouded sky, it does not seem certain that the author's explanation is correct. It seems hardly probable that the curvature of the earth can have much to do with the matter. In the first place, the curvature of the cloud-vault often seems much greater than that of the earth; whereas on von Zehender's theory it should, if anything, seem less, since the cloud-concave has a larger radius. And, secondly, the apparent concave can in all probability be fully explained as a peculiar instance of perspective. Any one who will observe the wall or ceiling of an exceeding long building, viewed from the middle so that the same plane runs to vanishing points both before and behind, will notice that the plane appears slightly to curve around the observer as it sweeps from one vanishing point to its polar opposite. The farther off one can get from the plane or line observed, the greater must be the apparent curvature as it passes the observer; thus the earth's surface even when viewed from a slight elevation seems to curve *upward* to the horizon, rather than downward as the actual curvature would lead one to expect. And let the eye be farther off, as when viewing the sea from some high headland, or from some mountain near the coast, and the apparent concavity of the surface becomes very striking; the waters mount upward toward the horizon and become veritably the 'high seas.' Similarly the cloud-stratum, viewed as it usually is from a con-

siderable distance—often several miles beneath—ought, on mere principles of perspective, to show the vault which our author and others would ascribe to a real curvature. They are merely bringing in a dubious factor, *præter necessitatem.*

The author's account of the sun-moon illusion is convincing, it seems to me, in the point that the illusion is not due to the low-vault appearance of the heavens; for it appears when the heavens do not seem low-vaulted. And he is probably on firm ground when he attributes the illusion to various factors. The least satisfactory part of his account is where he introduces the idea of converging meridians and Volkmann's illusion of perpendicular lines. If perpendiculars that are really parallel seem divergent above, would not this make the sun-moon illusion more difficult rather than easier to explain? For Volkmann's illusion by itself would lead one to expect that a disc of a constant size would seem larger the higher it went. And the suggestion of converging meridians does not seem more helpful. For assuming that the span between some given pair of meridians is taken as the unit of measurement, this unit is largest at the horizon and should as a result make the sun's disc seem small; and similarly the disc near the zenith where the lines converge would overlap more of these units than at the horizon and should therefore seem large, instead of small as it actually does. The more promising 'lead' in this problem appears to be found in the results of Zoth and Stroobant, showing that the direction of the line of regard influences our feeling for extension, so that of the equal distances, one looked at horizontally, the other from beneath, the latter seems the smaller. But even after all the objections that can be raised to particular points in his paper, one must gladly acknowledge that von Zehender's discussion of these two problems is by far the most illuminating that has ever been given.

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Ueber die mechanischen Correlate von Raum und Zeit, mit kritischen Betrachtungen über die E. Hering'sche Theorie vom Ortssinne der Netzhaut. E. STORCH. Zeitsch. f. Psych. u. Phys. d. Sinnesorgane, XXVI., 201–226.

In a fragmentary discussion of two pages, the mechanical correlate of time is asserted to be the continuous material changes of the brain, corresponding to the not further analyzable, most primitive activity of consciousness, which consists in the fading of each moment of a sen-

sory impression, even while the stimulus persists, into a memory image. This activity is immediately apprehended as the not further explainable *Time*, which is thus shown to be a necessary attribute of every sensation, even the first.

The mechanical correlate of space is a movement moment which is superadded to every sensory impulse. The psychic correlate of this movement moment is not to be confounded with the sensations of movement, since it never appears in consciousness by itself, and has absolutely nothing sensory in it. The author develops this theory of space without reference to its previous formulations or its classic difficulties, on the basis of a thorough-going psycho-physical parallelism. He supports it by an interesting and important case of monocular double vision without any physical cause. The patient developed strabismus in childhood. At nineteen the right normal eye was enucleated, and the patient reported that about 5° from every point fixated a second or 'illusory image' of the object appeared. Ophthalmoscopic investigation demonstrated that when this illusory image was fixated the retinal image fell on the anatomical macula, although it seemed to the patient that he was then looking beyond the real object. With the strabismus there had evidently developed an artificial macula. The author explains the double perception from a single retinal image by the development of a new system of local signs, in consequence of the shifting of the center of movement, while the original local signs of normal vision still persisted. A retinal image falling on the artificial macula is thus referred both to the center of vision and to a point at a distance from it, corresponding to the distance between the real and the artificial macula. The case deserves to be carefully followed and fully described.

The article is unfortunate in several respects. The only reference to any previous discussion of time is a misquotation of Kant, in which he is made to call time and space—'Formen reiner Sinnlichkeit' instead of 'reine Formen der Sinnlichkeit.' This is doubtless merely a slip, but it is an unfortunate one, for it looks like a total misunderstanding of Kantian terminology. Even more unfortunate are the general indifference to previous discussion and the thoughtless assumption and positive statement of debatable hypotheses, of which one at least, viz., the existence of a quantitative, non-sensuous consciousness of movement, has been proven false both by experimental investigation and by introspective analysis.

RAYMOND DODGE.

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On the Psychology and Physiology of Reading. II. EDMUND B. HUEY. The American Journal of Psychology, XII., pp. 292–312.

The article concludes the report of studies interrupted early in 1899. It contains a table showing the number of words read during one fixation in lines of varying length; and an interesting account of the rate of reading under various conditions of reproduction. The discussion of the unit of reading is a restatement of the theory of successive apprehension, agreeing in the main with the theory of Exner, without any consideration of the difficulties that theory has been shown to involve. The short account of the interpretative process of reading contains valuable introspective data.

RAYMOND DODGE.

The Perception of Visual Form. MISS L. HEMPSTEAD. Amer. Journal of Psychology, Jan., 1901. Pp. 185–192.

This paper is a preliminary report of experiments on the perception of gray outline figures placed on gray cards. These figures were seen through slits in a rotating disk which so controlled the illumination that figure and background were just perceptibly different from each other. Observers perceive such figures incorrectly, adding lines to complete symmetry and to fill out what seem to be, in many cases, purely individual constructions. Angles are seen much rounded and parts of the figures are vague, some lines being entirely omitted.

The writer evidently sees that these preliminary experiments have not dealt successfully with the distinction which will be of crucial importance for the interpretation of these results, namely, the distinction between the sensory factors which arise under the complex conditions here presented, and the perceptual processes resulting from these sensations. Later treatment of this phase of the problem is promised.

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Études de Psychologie. J. J. VAN BIERVLIET. Paris, Félix Alcan. 1901. Pp. 201.

The writer has presented in a series of popular essays his views on the structural and functional asymmetry of the human body, on the nature of the Müller-Lyer illusion, and on Flournoy's illusion of weights. He has also added an account of some reaction-time experiments tried in Wundt's laboratory in 1893. The book contains a few short tables of measurements, but is otherwise merely a restatement of well-known facts and conclusions. The chief significance of the

work is to be found in the concrete exhibition which it gives of activity in the psychological laboratory at the University of Gand.

CHARLES H. JUDD.

THE PERCEPTION OF MOVEMENT.

La perception des mouvements par le moyen des sensations tactiles des yeux. B. BOURDON. Revue Philosophique, 1900, L., 1–17.

Does the Sensation of Movement Originate in the Joint? W. B. PILLSBURY. Amer. Jour. of Psych., 1901, XII., 346–353.

A Comparison of Judgments for Weights Lifted with the Hand and Foot. A. J. KINNAMAN. Amer. Jour. of Psych., 1901, XII., 240–263.

Bourdon discusses the delicacy of the perception of movement with the eye when the latter fixates an isolated moving object. If a single luminous point is visible in a dark room, distant one-half meter from the eye, and is fixated as it moves, its movement begins to be perceptible with a rapidity of 2 mm. (14′ of arc) per second; the perception is sure with double this rapidity. The perception of the movement of larger luminous objects is slightly less delicate, because of less exact fixation. Intensity of illumination, and number of objects moving together, make no difference. When, however, a point moves in a field of visible immobile objects, the perception of its movement is much more delicate, being sure with a rapidity of 0.15 mm. (62″) per second. Aubert explained this difference by the assumption that a comparison is necessary between moving and not-moving objects, in order to give rise to the perception of movement; and that when the moving object alone is visible, it is compared with a representation of immobile space. Bourdon finds no trace of such a comparison when the experiment is actually tried. He concludes that the sensations giving rise to the movement-perception originate either in the eyelids or in the eye-muscles, or in both. That the eyelids play a large, perhaps preponderant, part is established by a series of experiments. By means of suitable apparatus a uniform movement of eyelid over eyeball was produced; the direction of movement was clearly distinguishable with a rapidity of displacement of 0.25 to 0.375 mm. per second, equivalent to an angular displacement of 1.19° to 1.79°. This sensibility is but slightly reduced on rendering the cornea anæsthetic. The tactile sensibility of the eyelids is not very delicate; a distance apart of 10 mm. is requisite for distinguishing two contacts. But other parts as little sensitive in this manner, as for example the

back of the hand, yet possess a very delicate sensibility for movement, where there can be no question of muscles involved, but where the skin can be moved freely over the underlying parts. The eyelids move to right and left, as well as up and down, in company with the movements of the eye; and their delicate perception of movement probably arises from the consequent distension of the skin. The muscles play an important part also, but this Bourdon has not investigated. To the argument of Volkmann that muscular and tactile sensibility of the eyes are not sufficiently delicate to account for our perceptions of movement and position, as proven by the fact that with eyes closed, we are very uncertain as to their position, Bourdon answers that the inexact convergence of the eyes under these conditions introduces some confusion; that there is a rapid diminution in tactile sensibility here as elsewhere when a given position is maintained; and that the perception of position is fairly exact when a single movement of the eyes and an immediate observation of the direction of regard is made.

"Since Goldscheider's work on the 'Muscle-sense' there seems to be very general agreement that the joint is the only or by far the most important source of the sensations that inform us that we have moved the members of the body." This view is now disputed by Pillsbury. The delicacy of perception of movement about the elbow and knee is diminished almost or quite as much by rendering anæsthetic by means of an electric current the wrist or the ankle, as by currents through the joints in question. This fact, together with the lack of anatomical evidence that the joints have sensory endings, makes it probable that the sensation of movement is derived mainly from the tendon and muscle rather than from the joint, inasmuch as these and these only are equally affected when the current is applied in the two places. It is also shown that the effect of the induction current in diminishing sensitivity cannot be due to any distracting effect produced by it.

Kinnaman's article gives the results of 9,000 out of a total of 18,000 tests made in judging weights lifted with the hand and with the foot. His standard weights were nine: 100 grams, 400, and by intervals of 400 up to 3,200 grams. They were lifted by aid of a band of cloth into which was inserted the hand or foot. This arrangement secured a more gradual lifting of the weights than the more rigid ap paratus ordinarily used, and gave considerable prominence to tactile sensibility in case of the lighter weights especially. His more important results are the following: (*a*) The weights were discriminated a little better through the hand than through the foot; (*b*) the relative

difference was greater for the small than for the large standards; (*c*) both showed a lower degree of sensibility both above and below 2,000–2,400 grams, or one-third of the reagent's maximum lift. The curves show at first a rapid increase of sensibility to about the 800-gram standard, followed by a less marked increase up to 1,600. Then they rise rapidly to the maximum, after which a slower decrease sets in. (*i*) The second test of the series is judged better than any others.

Among the introspective observations noted by Kinnaman are the following: (*b*) The judgment seemed to be based more on the changes in stimuli that occurred at the beginning of the lift, than on any constant sensations after the weight once cleared its base of support. The best results were attained by directing the attention to the first sensations. The basis of sensation appears to be the memory of a former change of sensation as compared with a present changing sensation. (*g*) One can distinguish quite noticeably, when lifting very large weights, that he is beset with a focal sensation, and with numerous marginal sensations, some having only a distracting effect upon the attention, such as noises, heat, the pains from uncomfortable positions and muscular pains, and others that must be regarded as auxiliary sensations to the focal sensation. They, with the focal sensation, figuratively, in a heap, constitute the real basis of discrimination. Among these marginal auxiliary sensations the following were pronounced: (1) Touch sensations around the grasp; (2) pressure sensations; (3) changes in the pressure of the back against the chair; (4) increased pressure on the seat of the chair; (5) feet pressing more firmly on the floor; (6) other hand putting forth effort; (7) interference with breathing; (8) interference with circulation; (9) intercostal and abdominal muscular sensation.

These marginal sensations are considered by Kinnaman to be of large importance in judging weights. The conscious basis of judgment was tactile for the 100-gram standard. The marginal auxiliary sensations, if present, were wholly subconscious. As the standard increases in weight, the marginal sensations loom up in consciousness more and more, and gradually the muscular sensations gain the ascendency. The weight of the arm gradually insinuates itself into the standard. With their cumulative influx, up to certain limits, the auxiliary marginal sensations probably increase the acuteness of discrimination. When very great, however, they may become sources of distraction, and then discriminative sensibility diminishes. It is probably impossible to isolate completely these various sources and bases of judgment so as to determine the comparative worth of each

for various standards, and within what limits Weber's, Fechner's or any other law holds. It may be that some law holds for each of the several bases after it becomes a factor in the discrimination, probably Weber's, but that no law has been or possibly can be formulated to apply to the sum of them.

In these three studies we have important contributions to the detail and theory of motor and allied judgments, and they cannot fail to stimulate further inquiry into these difficult problems.

E. B. DELABARRE.

BROWN UNIVERSITY.

SENSE OF TOUCH.

Die Raumschwelle bei Simultanreizung. ARTHUR BRÜCKNER. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Bd. 26, Hefte 1 u. 2, May 24, 1901. Pp. 33–60.

This paper reports a study of the conditions under which the fusion of touch sensations takes place. All the experiments seem to have been performed with very great care. Dr. Brückner constructed an æsthesiometer which enabled him to give absolutely simultaneous contacts and to regulate the force of the impacts. Most of the experiments were made with the points from 2 to 30 mm. apart. He found that when two points seem like one the sensation produced by the double stimulus is stronger than that produced by a single stimulus, and that two impacts, each so faint as to be imperceptible in itself, may be perceived when given together. This he explains physiologically, as due to summation. The percentage of cases of apparent summation at the various distances within 30 mm. is nearly the same. The average is 81.5 per cent. In order to bring out the summation results he finds that the subject must give his attention wholly to the strength of the stimulus and not interest himself in the perception of two points.

The most interesting part of his article, perhaps, is that in which he develops a new method of obtaining a threshold. Instead of seeking the distance at which two points can be distinguished, he suggests finding how far apart two stimulations, either of which by itself would be imperceptible, may be and still fuse into one perceptible sensation. However, various factors, chiefly psychic, affect the judgments so that the distance does not remain constant. The double stimulus when perceived as one is often located at a point different from either of the two points actually touched, and when the distance of the two points is relatively great it is often located between them.

J. F. MESSENGER.

COLUMBIA UNIVERSITY.

DISEASES OF ORIENTATION.

Les maladies de l'orientation et de équilibre. J. GRASSET. (Bibl. scient. intern.) Paris, F. Alcan. 1901. Pp. 291.

This exhaustive study of the disorders of orientation and equilibrium consists of material gathered for a series of lectures recently delivered at the Université de Montpellier.

The discussion is divided into six parts. In Part I. a number of typical cases are described, with symptoms covering the various manifestations of these disorders; all the cases chosen were within the author's personal observation, most of them being actually demonstrated in connection with the lectures. Part II. is a survey of the nerve courses and centers concerned in the functions under discussion. Part III. is a brief classification of the disorders of equilibration, a term which the author uses to embrace the two functions of equilibrium and orientation. In Part IV., which comprises nearly two-thirds of the work, the symptoms are presented in systematic detail, as a result of which the seat of disorder in the nervous system is worked out for the several cases and embodied in a schematic diagram in Part V. Part VI. is a brief discussion of the physiological treatment of the disorders in question.

A large portion of the material presented in this book, and that which is more especially of interest to the psychologist, has already been published by the author in an article in the *Revue Philosophique* for March and April, 1901, a résumé of which was given in this REVIEW for September (p. 529). While this has been very much extended in places, the additions are generally for the benefit of the clinical student, and the discussion in the *Revue Philosophique* is probably better suited to the general reader. The amplified schemes, diagrams and tables which are given in the book, however, deserve the attention of those interested in the physiology and pathology of these functions.

H. C. W.

EPISTEMOLOGY AND ETHICS.

Essai critique sur le droit d'affirmer. ALBERT LECLÈRE. Paris, Félix Alcan. 1901. Pp. 263.

In this essay the author has attempted a rehabilitation of the teachings of Parmenides and the introduction of the old Eleatic spirit into the modern controversies concerning the problems of ontology and epistemology. He maintains, quite dogmatically it would seem, that the sole source of certitude is in the region of pure thought, or 'thought in itself,' which lies above and distinct from the lower and uncertain

level of empirical consciousness. The real, accordingly, is that which evidences itself through the sheer force of self-affirmation. The unreal lacks this self-assertive coefficient. We come, therefore, to deny reality to every concept which appears under contradictory aspects. The fundamental law of thought is the law of identity; it, moreover, is perfectly and satisfactorily comprehensive. From this fundamental postulate the author deduces the unreality of the phenomenal world, and, inasmuch as science has to do exclusively with the phenomenal, it must, therefore, be a science of the unreal. In other words, the phenomenal world necessarily resists all attempts at unification. There can never be a metaphysic of science. The phenomenal must be received merely at its face value. A further interpretation, a deeper significance, a final generalization are alike impossible.

The real, therefore, is that which lives, moves and has its being in the realm of pure thought. The essence of reality is personality and the essence of personality is freedom. Hence, it is possible to deduce the being of God, and God thus conceived becomes the norm of reality, and the supreme object of religion, which is the necessary complement of reality. Such, in the main, is the rough outline of the system. The author's point of view, it seems to me, presents the evident limitations of a too refined abstraction; his thought, therefore, lacks that wealth of content which only the concrete can give. There are two ways of rising to the higher level of pure thought, one by leaving the lower level absolutely and thereby denying its reality altogether, the other by so sublimating the lower that it preserves and manifests its reality in the higher. The author, it seems to me, labors under the disadvantage of pursuing the former of these two methods rather than the latter.

JOHN GRIER HIBBEN.

PRINCETON UNIVERSITY.

The Utilitarian Estimate of Knowledge. PROFESSOR JAMES SETH. The Philosophical Review, July, 1901, pp. 342–358.

This article is of special interest at the present time in the light of the recent publication of Mr. Leslie Stephen's 'Utilitarianism.' The difference in point of view is of course a radical one. Professor Seth's insistence upon the value of knowledge for its own sake, irrespective of its utility, is a most timely service. His conclusions may well be taken as an antidote to the doctrines of Mr. Stephen, as expressed in his 'Utilitarianism'; at least, they fulfil the function of emphasizing the fact that there may be another side to the question, and that it has a case which merits at least a considerate hearing.

Professor Seth's position is briefly as follows:

There is such a thing as knowledge which has no instrumental value, and yet carries with it a worth of its own. It is to be assessed not by any external standard, but by one which lies within. Much of knowledge is for the will, but not all knowledge. If it is regarded as having merely a practical value, it tends to lose even that value. There is here a paradox which is similar to that of hedonism. Moreover, the reflex influence of the disinterested pursuit of truth upon the scholar himself has an ethical value which can not be too highly estimated. The discussion is, however, generally epistemological rather than ethical.

The article abounds in historical allusions to the various systems of philosophical thought which have failed wholly or in part to allow a non-utilitarian factor in knowledge, notably the undue estimate on the part of Kant of the importance of the practical reason.

This plea for pure science is one which merits very grave consideration, whatever may be one's particular views concerning the general doctrine of utilitarianism. The tendency of the present day thought is to emphasize unduly the art of knowledge and to overlook completely, in certain quarters at least, the science of knowledge. This fact has a marked pedagogical significance. The average student to-day in our universities, if not the whole student body, determines his choice of studies in a large measure by their supposed utility in reference to the work or profession of life; and interest in intellectual pursuits rises and falls with barometric exactness according as there may or may not be evidence of the possibility of some practical application. This is but one of the many indications at the present time, that these exclusively utilitarian ideas are in the air, and have become all-pervasive. It seems to me that the essential characteristic of a scholar is his devotion to some form of pure knowledge for its own sake and that as there is a categorical imperative in ethics, so likewise there is a categorical imperative in scholarship. And in so far as the scholar comes to pursue knowledge with an eye askance as to the benefits which may accrue either to himself or the world at large, he is so far forth less worthy of the name of scholar.

JOHN GRIER HIBBEN.

THE EMOTIONS.

Les Timides et la Timidité. PAUL HARTENBERG. Paris, Alcan. 1901. Pp. xv + 264.

Those who have any regard for the 'old' psychology are sufficiently warned in the preface that this book is not for them. It is a monograph in scientific psychology; and this is neither more nor less than

a study of brain-functions. On the other hand, those physiologists who have taken endless pains to find out something about cerebral functions, must confess that Dr. Hartenberg's method has the charm of novelty. With the exception of one plethysmograph tracing, there is no evidence in the book of experimental work. In fact, the author says it would not have given us any new information. This may be true; it may even have occurred to those antiquated psychologists who imagined that there was more for them to study than the functions of the brain. But they would not have made amends for the lack of experiment by such lengthy and well-chosen extracts from Amiel, Stendthal, Rousseau and Bourget as are offered in these pages. The value of these extracts from various departments of literature lies in the fact that they show the results of introspection and careful analysis. The author deserves credit for having arranged them under suitable heads and using them to illustrate his definition of timidity and his description of its symptoms.

Timidity is a combination of fear and shame—both groundless—which is felt in the presence of other persons. Its symptoms are, on the organic side, trembling, blushing, disturbances in speech and in the visceral and secretory functions. These are accompanied, on the psychical side, by derangements in the processes of attention, reflection, volition and memory.

The most satisfactory chapter in the book is the third, entitled 'Le Caractère des Timides,' or, on some pages, 'Caractère des Timides.' A distinction is made between the primary qualities of timidity and the modifications which it produces. The former include sensitiveness, fear of ridicule, scrupulosity and a certain secretiveness which is due, not to reserve, but to the dread of being misunderstood. The resulting modifications show a peculiar blending of opposite traits—misanthropy and benevolence, humility and pride. Further consequences are egotism and dilettantism, repression of the feelings and, eventually, inability to express them. In some cases, this self-restraint is followed by an explosive reaction, a sudden display of courage and energy which, for the time being, are without bounds. Either the primary qualities or the secondary modifications may be exaggerated or complicated to such a degree that the health of the patient is impaired—pathological forms of timidity result. These as well as the milder types may, like other diseases, yield to treatment. The patient himself should be urged to cultivate sociability; and various means are suggested by the author. These, in morbid conditions, are to be supplemented by medical treatment.

CATHOLIC UNIVERSITY, WASHINGTON, D. C. E. A. PACE.

HYPNOTISM.

Somnambulism, Hypnotism, Suggestion and Kindred Questions before the Fourth International Congress of Psychology. Compte Rendu des Séances.

To these problems the third general session of the Congress and the three sessions of Section V were entirely devoted. Many of the papers presented are given by title only or in abstract in the *Proceedings* and must therefore be left out of consideration for the present. Many of the others hardly deserve better treatment. None of those whose subject matters fall within the pale of orthodoxy present either facts or theories of any considerable significance. Even Professor Flournoy's contribution to the history of Mlle. Hélène Smith goes but little beyond what he had already told us in his monograph.

The 'kindred phenomena'—which seems to be a screen for the 'psychic research' bogey—fared but little better at the hands of their would-be champions. Three papers of real interest were read, all dealing with Mrs. Thompson, an English automatist, seemingly of the type represented by Mrs. Piper. In one Mr. F. W. H. Myers sums up the phenomena he has observed in her case and the conclusions to which he has been led. In a second Dr. Van Eeden gives some facts, discusses the alternative theories, and concludes by admitting (p. 130), "I cannot doubt that for some minutes I had to do with the voluntary manifestations of a deceased human being." But this conviction attaches in his mind to but a very small part of the phenomena. A large part of the remainder he ascribes to the histrionic gifts of Mrs. Thompson's subliminal self. The third paper, by Mrs. Verrall, is more interesting because it gives more facts, but she does not attempt to base any theory upon them.

Alfred Grafé describes at length some experiments upon a supposed clairvoyant. But the descriptions of the conditions under which they were performed is not detailed enough to enable one to judge whether the possibility of fraud was excluded.

Dr. John E. Purdon briefly reports cases observed by him in 1881 and 1882, in which the pulse-wave of one patient seemed to be telepathically transferred to another, the patients giving identical tracings. It is to be regretted that no more careful and detailed record was made—or published—of these cases.

None of the remaining articles call for special mention. Some were utterly unworthy of the occasion. "Ce sont," says one speaker, "des impressions littéraires, des confessions, de certaines professions

de foi, mêlées avec une regrettable ignorance des documents scientifiques, enregistrés dans la psychologie depuis des années" (p. 617). It is to be regretted that the tolerant spirit displayed by the organizers of the Congress in granting a hearing to the representatives of views with which few of them had any sympathy should have been in some cases so ill rewarded.

W. R. NEWBOLD.

UNIVERSITY OF PENNSYLVANIA.

NEW BOOKS.

The World and the Individual. JOSIAH ROYCE. New York and London, The Macmillan Company. 1901. Pp. xx + 480. $2.25.

Inductive Sociology. FRANKLIN HENRY GIDDINGS. New York and London, The Macmillan Company. 1901. Pp. xviii + 302.

The Evolution of Sex. PATRICK GEDDES, J. ARTHUR THOMSON. London, Walter Scott. 1901. Pp. xx + 342. $1.50.

The Limits of Evolution. G. H. HOWISON. New York and London, The Macmillan Company. 1901. Pp. xxxv + 396.

The Adversaries of the Sceptic. ALFRED HODDER. London, Swan Sonnenschein & Co., Ltd.; New York, The Macmillan Co. 1901. Pp. 339.

G. T. Fechner. W. WUNDT. Leipzig. 1901. Pp. 92.

NOTES.

THE American Psychological Association has fixed the first day of its Chicago meeting for December 31st. The Western Philosophical Association will this year meet in conjunction with the American Psychological Association.

THE next International Congress of Physiologists will be held at Brussels in 1904, under the presidency of Professor Heger.

THE Hon. Oscar Straus has given to the University of Georgia several hundred dollars toward an equipment for work in experimental psychology.

THE lectureship in connection with the California Philosophical Union for the current year has been offered to, and accepted by, Professor R. M. Wenley, of the University of Michigan.

CLARK WISSLER, A.B. (Indiana), Ph.D. (Columbia), has been appointed instructor in psychology in the School of Pedagogy of New York University, and Dr. J. E. Lough has been promoted from an instructorship to an acting professorship.

DR. ADOLPH MEYER, director of the clinical work and laboratory of the Worcester Insane Asylum and docent in psychiatry in Clark University, has been selected as director of the Pathological Institute of the New York State Hospitals.

THE sum of about $12,000 has been subscribed for the memorial in honor of the late Professor Henry Sidgwick. It will be used for the establishment of a lectureship in moral science in Cambridge University.

DR. ARTHUR KÖNIG, associate professor of the physiology of the sense organs at the University of Berlin, and director of the physical section of the Physiological Laboratory, died on October 26th, at the age of forty-five years. Dr. König was an assistant of Helmholtz's and aided in the preparation of the second edition of the 'Physiologische Optik.' He had carried out important researches on vision, and, with Professor Herm. Ebbinghaus, edited the 'Zeitschrift für Psychologie und Physiologie der Sinnesorgane.'

A. A. TOKARSKY, head of the Moscow Psychological Laboratory, died on July 22d.

DR. ALEXANDER HUGHES BENNETT, known for his work on diseases of the nervous system, died in London on November 1st, at the age of fifty-three years.

R. KOENIG, of Paris, well-known for his researches on acoustics and for his acoustical instruments, has died at the age of sixty-nine years.